THE

WESTERN JOURNAL

OF

MEDICINE AND SURGERY.

OCTOBER, 1850.

ART. I.—*A Report on Typhoid Fever made to the Scott County Medical Society.* By W. L. SUTTON, M. D., of Georgetown, Ky.

(CONTINUED.)

CHAPTER IV.

ARTICLE 1.

DURATION AND PROGNOSIS.

There is much difficulty in determining the duration of many cases of fever. Very frequently the advent is so insidious that its presence is not noticed until the patient becomes seriously ill, and when the physician sees him, he is not able to give any satisfactory account of his previous state or feelings. Again, for several days, the

change for the better cannot be appreciated so as to say with any assurance on what day it commenced. Add to this, that in a given proportion of cases, there is an apparent but probably no real amendment, which is succeeded by an aggravation of symptoms and fatal termination.

It seems to me that the time at which convalescence is decided by Dr. Jackson to have commenced, viz.: "When the patient is able to take a moderate allowance of solid food, the febrile symptoms having subsided for at least two or three days previous to this period," is somewhat objectionable. I think it should be dated from the time when amendment is decided. It seems to me too, that it is not safe to allow solid food for several days, say a week after amendment is evident.

Keeping these obstacles in mind, I will state that on the 6th of July, 1846, Dick, a negro boy eighteen years old sickened, not being known to be ill before; he died on the 12th. Mrs. S. sickened on the 14th of July, having been a good deal indisposed for two weeks, and having lost much flesh; she died on the 20th. These, I think are the shortest courses of fatal cases, which have fallen under my observation. Whether Dick had been indisposed, but chose to conceal it, I do not know. His attack seemed to be sudden and exceedingly severe. I saw him on the second day of his disease, when his pulse was one hundred and thirty, great distention and tenderness of abdomen, profuse diarrhea, and delirium. I was visiting the house daily when Mrs. S. gave up; she had no fever or diarrhea before her attack. In fact I had made some laxative pills for her the day before her attack, of which however she had not taken any.

J. A. R. aged eight had been complaining of debility two weeks, sleepy, and heavy; was taken April 30, 1847, at night; chilly, afterwards feverish. May 1st—vomited some bile, soon after which I saw him; face red, eyes watery and injected; tongue whitish with red papillæ, lips

vermilion; no diarrhea, slight meteorism; gurgling at the right iliac region; pulse one hundred and twenty. ℞. Ipecac. grs. xv. Got very little of it on account of vomiting every time it was put to his lips—threw up a little bile, no phlegm. Within a few minutes after taking ipecac. had a liquid fetid cider stool, and another in fifteen or twenty minutes. ℞. Cal. mass. hydrarg, aa. grs.; iv. pulv. Dover's p. grs. iii. nocte. 2d. Had no stool since I left until this morning, when he had three, much as those of yesterday, pulse 110, otherwise as yesterday. ℞. No med. 3d. Appeared so much relieved that I prescribed nothing and ceased attendance. 7th. Saw him on the farm where his father was at work. This may not be considered a case of typhoid fever, or it may be considered one which had remained latent. However it may appear to others, I certainly, at the time, thought it a well marked case; one that was likely to give me trouble. With the exception of a few such cases as the preceding, I have found those presenting such symptoms go on for two or three weeks.

I saw John Sutphin on the 20th of June, 1846, he having been sick three weeks; he died 12th July, making six weeks illness. This is the longest case that I have attended regularly, and this only during the latter half of his illness. I have seen one case however who was confined twelve weeks or upwards, a large portion of that time being in a very precarious situation. Such may be considered the extremes of cases seen by me.

In general there is very gradual aggravation of symptoms for six or eight days; then there are from four to eight days, during which no appreciable change takes place. Then there is a very gradual amendment for five or six days more, and then a more rapid improvement.

It would seem that the severity of the attack has very little to do with its duration. How mild soever it may be, it is apt to last two or three weeks. For instance I was myself confined during the last summer, (1847,) for

five weeks with, as I think, typhoid fever. There was scarcely a day during the time, when I did not feel as if I were well enough to ride twenty miles; yet I could not sit up with comfort half an hour. After the first few days, I scarcely had any appreciable evidence of disease; and my brethren who visited me frequently, saw no indication for medicine. One of them remarked one day rather dryly, "that if he were lying in bed so long without more manifestation of disease, he should think he had the hypo." After the first few days my pulse ranged from sixty to eighty-five; yet during the fifth week, owing to a very slight indiscretion in eating, and sitting up long enough to write a letter of half a page, my pulse rose up to one hundred and twenty.

Circumstances like this go farther to support the idea of a specific character of this disease than any thing else that I know of.

ARTICLE 2.

PROGNOSIS.

The prognosis of bad cases is made out with considerable difficulty. We may have some very grave symptoms united with others of better omen, viz.: We may have a frequent pulse, perhaps joined with a dry, smooth tongue, but the mind may be clear; the expression of countenance good; the bowels not much vexed with diarrhea, little distention or tenderness of the abdomen; no subsultus tendinum. Again, the pulse may be quick with considerable distension and tenderness of the abdomen; but the mouth moist, little diarrhea; soft skin, no delirium, &c.

Great frequency of pulse, long continued profuse diar-

rhea, quick respiration, especially if performed irregularly, dry, black tongue, profuse hemorrhage at the latter stages of the disease; subsultus tendinum, low muttering delirium; loud raving; great prostration; discharges of flatus; stranguary, suppression of urine, restlessness, and an impression of being well when very grave symptoms are present, are among the symptoms indicating great danger. If I were to select three symptoms indicating much peril, I would take a frequent weak pulse, profuse and continued diarrhea, and a dry, black tongue. Whatever other symptoms may be present, I hold that a combination of these is of evil import. I do not now remember a case of doubtful aspect, in which one or more of these symptoms were not present; and the converse is true. I do not remember a case in which the three were united which was not a grave case. As one or more of these three, united with other symptoms enumerated as indicating danger, are present so will the danger of the case be.

We must remember however, that there is no disease in which there is more reason to hope, against hope. Thus in a case in which there had been continued diarrhea, with quick pulse, and great muscular prostration, profuse epistaxis came on in the last stage, and death seemed to be averted only by plugging the nostrils; yet recovery was at last rapidly effected. It may be proper to notice a case which I saw in 1849. A young gentleman had been very low for more than a week. Among the grave symptoms was free hemorrhage from the bowels. After one of these discharges he sank very much, pulse became very frequent and too weak to be counted, and it seemed as if death must speedily ensue. He seemed at least as low, if not lower than the last case. Yet he recovered notwithstanding the continuance of hemorrhage from the bowels for twelve or fifteen hours after.

Again, in a case of extreme prostration, profuse diarrhea, delirium, and where in truth the patient was abandoned by his physician, the pulse however remaining tolerable steady, recovery took place. Thus it is that in all cases we must lean to the side of hope. On the other hand we must not forget that there is another side to the picture. In a few cases in which the disease has apparently been mild, when in fact no very grave symptom has shown itself, when the convalescence is considered as already set in, a perforation of the bowel may take place, and death speedily close the scene. I have seen a case in which for four weeks the pulse ranged from sixty to eighty; the bowels never much troubled; the tongue nearly natural; no undue heat of skin; indeed no bad symptom occurred, if we except a moderate but obstinate pain in the occiput. During all this time the patient set up more or less every day, generally in a room adjoining his bed-room, yet at the expiration of this time he began to become deaf, his pulse slowly increased in frequency, his tongue became gradually coated and dry; presently delirium set in, and during the sixth week he died.

Again, a case may be severe, but improvement may appear to have commenced and continued for two or three days, when an aggravation of symptoms may take place, and the patient steadily grow worse, until a fatal termination ensues. This aggravation may occur without any cause apparent to the physician, or nurse. It is frequently charged upon improper indulgence. If this last has been the cause, we may hope by diligence and attention to remedy the evil, but if it is not, the prospect is truly gloomy, as I do not remember a case of recovery from this state.

I have found profuse hemorrhage in the last stage, much less fatal than I was prepared to expect from reading or reflection. I remember but ten cases in which it

occurred. In two only did death follow.* In two cases where I was satisfied a perforation of the bowel had taken place, and when the patient evidently was to live but a few hours, each was impressed firmly with the idea that nothing was the matter.

I have found stranguary and suppression of urine, both to be symptoms of evil import. The patient, from whom it was necessary to draw the urine daily for six weeks, was the only case of recovery in which either symptom was observed.

The case above mentioned, in which a general rigidity of the muscles occurred, recovered. I ought perhaps to state that there was no permanent rigidity, but it came on when the body was to be moved. It continued to recur for thirty-six or forty-eight hours.

CHAPTER V.

APPEARANCE ON DISSECTION.

Dissections of patients dying of this affection, have not been made, in this vicinity at least, at all commensurate with the gravity of the disease, or the interest of the profession. Doubtless, dissections have been made—but in 1841, I made my first post mortem examination; and at that time, I was not aware that any proof of identity between this disease and the typhoid fever of France and New England existed, save in the symptoms during life. Indeed, I made a special inquiry of Dr. Holland, at the time he was called to see my first lot of cases, if any dis-

*Since writing the above I have seen several other cases but no death.

sections had been made in Lexington or its vicinity, to determine the point, and he told me that he was not aware of any. I have made but three autopsies; two patients having died about the fifteenth day, the other on the thirty-first. I have also a cast of a portion of the ilium, taken by my son from a patient who died at the end of the third week.

A. Stubbs, aged twenty-three, had been sick fifteen days, during which time great general uneasiness, considerable pain in the abdomen and throat, excessively dry, cracked tongue, and coolness of the extremities, were the most marked symptoms. Meteorism, or diarrhea was never considerable; pulse for a week before death one hundred and fifteen to one hundred and twenty-five; no rose spots; no sudamina until the day of his death, when a crop appeared upon his face and neck. Died March 15, 1846.

Autopsy twenty-three hours after death.—Muscular contraction very rigid; abdomen not distended; the skin as hard to cut as parchment; [a blister had been applied on the abdomen on the 5th;] the omentum very much injected and extended down to the pubis; the bowels, when exposed by raising the omentum, appeared considerably injected, their calibre diminished. *Liver*, apparently healthy. *Stomach*, externally of good appearance, internally considerably injected below the middle depression, and empty, except that the surface was covered with good looking bile. *The intestines* throughout were lined with the same bilious matter, otherwise empty; the small intestines red internally throughout their course; near the ilio-cecal valve they were more highly colored externally, and internally presented elliptical plates distinctly elevated and some of them ulcerated in numerous distinct minute points; solitary glands distinctly thickened, and some of them ulcerated; the mucus membrane considerably injected, having veins distended with dark blood inter-

spersed. *Colon*, externally many blood vessels; the calibre very much diminished, cord-like, the lateral pouches being nearly obliterated. The spleen, pancreas, and kidneys, not examined; the thorax not opened. In fact we had to make the examination surreptitiously, and in the house where there were more lying very ill, and, of course, imperfectly.

2d. Ben, aged twenty-five, had been sick about two weeks; profuse diarrhea, mostly of a dirty black green. At first considerable pain in abdomen, subsequently but little; considerable tympanites; tongue furred, whitish; pulse from seventy to eighty, never became frequent. During his illness he vomited a dark green bile, but not enough for it to be considered a prominent symptom. There had been great muscular weakness; but Dr. C., who was kind enough to invite me to witness the autopsy, informed me that he did not consider the case at all dangerous until a very few days before death.

Autopsy.—Considerable emaciation. *Abdomen* distended; upon laying it open the whole of the abdominal viscera were of a uniform color, difficult to describe, perhaps a leaden color tinged with dark green, would be as good a phrase as any other. This color extended to the peritoneum lining the abdominal walls. *Liver* was of a healthy color on the convex side, on the concave was darker than the color of the bowels; an incision into its substance showed a healthy structure, so that its color probably depended upon the shade given to the general color of the bowels by the darker one of the liver. Gall bladder distended by dark green bile *Stomach* very small, apparently healthy, but not opened. Duodenum of good calibre, not opened. Jejunum empty, less colored than the other viscera. Ileum well distended with air, and dark green bile; a portion taken from the upper part of it, showed the mucus coat somewhat injected, otherwise healthy; no part of the ileum, or indeed any of the bow-

els exhibited any thickening of its coats, or any contraction of its diameter; but upon removing fifteen or eighteen inches of the lower portion of the ileum, emptying it and washing off the bilious matter, several elliptical patches and solitary glands were found slightly ulcerated, without apparent thickening or diminution of the walls. Colon considerably distended. Spleen enlarged, almost globular, very red and much softened, breaking down under the handling, and a considerable quantity of blood was lost from the vessels ruptured in extracting it. The pancreas and kidneys not examined. *Thorax*, heart and lungs healthy.

3d. Sylla, aged forty-five, sick thirty days. Died August 19, 1841.

Autopsy twenty hours after death.—About one inch of fat covered the abdominal muscles; omentum well down in the pelvis; stomach moderately distended with air and a little fluid; externally and internally healthy, if we except a small spot just below the central depression, which exhibited a group of innumerable minute red specks, as if the point of a fine needle had been repeatedly applied. The small intestines in various places appeared diminished in volume, otherwise sound until we approached the lower end of the ileum, where externally it was dark and contracted. Upon laying it open, a number of ulcers were to be seen varying in size, some larger than a grain of wheat, others larger than a twenty five cent piece. They extended to the peritoneal coat. In some of the smaller ulcers, the edges were thickened, in the larger there was no thickening of the edges. The intermediate spaces were filled with enlarged veins containing dark blood. Near the ileo-cœcal valve there were some large very irregularly shaped ulcerations. In the cecum and upper part of the colon, were similar ulcers, also near the anus. The colon externally was of a healthy color, but very much diminished in calibre; internally

throughout, as dark as a mucus membrane could well be. The rectum in the same condition. In the bowels was a small quantity of semifluid yellowish fecal matter. Some of the mesenteric glands enlarged. There was very little blood in the vena cava or any of the large veins. Indeed there appeared a very considerable scarcity of blood in the cutaneous veins and those in the cavity of the body. *Liver and pancreas* healthy. *Spleen enlarged* and softened. Bladder and uterus apparently healthy externally, but neither was opened.

CHAPTER VI.

ARTICLE 1.

DIAGNOSIS AND NATURE.—GENERAL REMARKS.

"Our diagnosis can never be founded here, as in many other instances, on a few positive physical signs. It must always be rational, not absolute. The evidence upon which our verdict is rendered is wholly circumstantial. Notwithstanding all this, and though cases sometimes occur so enveloped in obscurity as to baffle the skill of the most careful and experienced observers, it is still true, that there are few general diseases the diagnosis of which is so well established, and so certain as that of typhoid fever. * * * Setting aside, as I do for the present, the true typhus fever, there is no disease more readily and positively recognised than a case of well marked typhoid fever, of extreme or even of average severity, when observed from its commencement and followed through its entire course."—*Bartlett on Typhoid and Typhus Fever.*

Just before the quotation first above made, the author informs us that there are no two or three symptoms which show the presence of this disease—that there are no two or three symptoms which may not be absent during the whole course of the disease. He might have made his statement even broader than that. A majority of the symptoms even of those considered most characteristic of the disease may be present and yet the disease absent and *vice versa*.

I shall not go over a repetition of all the symptoms which may be looked for in this disease, but only some of those which I consider of most importance, presuming that the patient is seen during the first week. A tongue whitish, moist, or beginning to become clammy with red papillæ showing through the white coat, and reddish edges and tip; giddiness and tinnitus aurium upon rising; more or less uneasiness of the head; tenderness at the epigastrium or right iliac fossa; tympanitic state of the abdomen, diarrhea, the dejections of a dirty yellowish color, very liquid and very offensive, of a cadaverous odor, with or without griping; great muscular debility; a pulse increased in frequency, it may be increased or diminished in volume and strength; occasionally a cadaverous smell of the body. Although, as before observed we may have a case of typhoid fever with a majority of these symptoms wanting, or a majority of the symptoms without the fever, yet when we have something like the assemblage just enumerated we should be on the look out for the disease. I will offer a few remarks upon the value of each of these symptoms.

The tongue above described is such as I have most frequently seen in recent cases of fevers; but it is also found in cases of intestinal irritation no matter from what cause it may arise. In measles and scarlatina at the commencement, it is very much of the same character.

Giddiness and tinnitus aurium may attend gastric irri-

tation or debility from whatever cause. Tenderness of the abdomen may attend gastritis or enteritis. Tympanites may attend peritonitis, pneumonia, dysentery. Diarrhea, with discharges as described, I at one time considered nearly pathognomonic of typhoid fever, but I have seen it in pneumonia, in measles, and bilious fever and cholera—indeed, during the last few years it has come to be very common in most of our diseases.

Great muscular debility attends many forms of disease in nervous temperaments. With the same amount of accompanying symptoms however, I think the debility in this disease is greater than in any which I have observed. Dr. N. Smith considered a cadaveric smell of the body as pathognomonic; believing that if introduced blindfolded into a room where the disease was, he could detect it. Professor Bartlett seems to be of the same opinion. I will add that during the last summer, a gentleman, whose family had suffered severely in the East with fever, was introduced into the room of a patient, suffering with the disease, who immediately recognised the odor. Still there is frequently some difference between what a man thinks he can do, and what he can actually do. For example: during the winter of 1818–'19, whilst attending a course of lectures in the University of Maryland, my room-mate became sick and had the attendance of Professor Potter, who assured him he had measles. Mr. Sanders insisted that could not be the case, because he had had both regular and French measles. Professor P. however had more confidence in his own diagnosis than in that of his patient, and replied that if he had had forty kinds of measles, the eruption would appear in the next twenty-four hours—that he had no difficulty in detecting the measles by the smell of the patient. Upon his next visit, sure enough, he found the eruption—not of measles as he acknowledged, but of typhus fever. And if I could depend upon my recollection for twenty-eight years, I would say

that eruption corresponded precisely with my idea of the rose spots of typhoid fever, and the only spots I have ever seen which did.

By the way, I remember distinctly his teaching that in typhus fever there was always inflammation of the bowels, but I do not remember that he located that inflammation in the ileum; or that he taught that ulceration was common. On the contrary I have always been under the impression that he and Broussais, both taught that the inflammation was in the stomach and duodenum. How true my impression of either case is, I do not know, or whether my notion is not founded on a somewhat natural interpretation of the term gastro-enteritis.

But to return from this digression, I have observed this odor in many patients, ill of typhoid fever—in many, I have not detected it. I have also noticed it in patients laboring under various other diseases.

ARTICLE 2.

DIAGNOSIS FROM BILIOUS FEVER.

A person who has been in the habit of treating remittent fever will, upon seeing a case of typhoid fever, readily perceive that there is something of an unusual appearance in it. He may not suspect it to be a fever of a different type, and may have difficulty in ascertaining what causes operate to produce the anomalous train of symptoms. He may even have some difficulty in identifying the anomalous characters, but still he is sensible that the case is one varying materially from such cases as he has been in the habit of seeing. It is true that there are cases which, appearing to partake of the nature of remittent bilious fever and of typhoid fever, may be classed

with difficulty; such cases occur respecting most diseases at all analogous to each other; but in the vast majority of cases there can be little difficulty in determining in what list to place a given case. The first distinguishing feature which I shall notice is the period of prevalence.

1. Typhoid fever occurs at all seasons indiscriminately. It is true that in some years it will be found to prevail mostly during the fall or summer; but in other years its period of prevalence will be winter or spring; and very generally it will rage with much the same intensity throughout the year. Bilious remittent fever on the other hand may show, it is true, an occasional case in the winter or spring, but rarely if ever prevails extensively at either period, but rages with violence more or less marked, in July, August, September, October, and November.

2. A second distinguishing feature between these two diseases is the liability to return—relapses. Relapses rarely take place in typhoid fever. By relapse I mean a falling into a state essentially the same as that which existed in the original attack, *e. g.*—if during convalescence from measles, a patient is exposed to cold, and catarrhal symptoms attended with fever come on, I do not call this a relapse; but if during convalescence from bilious remittent fever, a patient from any cause, has a return of his paroxysms of fever, either in the remittent or intermittent form, that I call a relapse; because although there may be great difference in the severity of the paroxysms and of the intervals of return, or even of the trains of symptoms which constitute the paroxysm, yet the latter attack is essentially the same as the first. In this sense a relapse rarely takes place in typhoid fever. On the other hand this disposition is one of the most strongly marked characteristics of remittent fever. Indeed a person who has had intermittent or remittent fever, cannot be pro-

nounced free from a liability to relapse, for a year afterwards.

3. In the cold stage of the two fevers there is a marked difference. In a large majority of cases of typhoid fever, the cold stage it not very distinctly marked; a chilly sensation of greater or less duration being somewhat common, while many cases are not attended by any chill which has fixed the attention of the patient. It is true, that in many cases of remittent fever, there is but a moderate chill at the onset; this chill in such cases being about as severe as those occurring with most severity in typhoid fever; but the access of bilious fever is usually marked by a severe rigor, amounting to decided shivering and chattering of teeth, which it is impossible for the patient to control. This chill, rigor, or ague, is less and less distinctly marked as the disease progresses until it becomes scarcely appreciable.

4. There is a very marked difference in the manner of recurrence of the cold stage. When that stage is repeated in typhoid fever, it is at uncertain intervals and rarely amounting to a distinct rigor, but rather an indefinite sensation of discomfort. In remittent fever, the rigors return with astonishing regularity. It may be that the paroxysm returns every day, or it may be every other day, or at some other interval; but whatever that interval is, it can be soon ascertained. Again, it may return at the same hour at every revolution; or it may return an hour sooner, or an hour later at each period. Having these elements of computation, we are enabled, after carefully noting two or three paroxysms, to predict within fifteen minutes, the time at which the next paroxysm will appear.

5. In typhoid fever, the hot stage comes on very gradually, and the heat of skin, fullness and frequency of pulse are rarely very great in the first days of disease;

subsequently the heat of skin abates, but the frequency of pulse increases. In bilious fever the transition from the cold stage to the hot is abrupt, the pulse becoming full and bounding and the heat of the skin great.

6. There is no regular sweating stage in typhoid fever. It is true that the skin occasionally becomes more or less moist, nay there is sometimes a real perspiration, but the occasions upon which this last is observed are rare, and no one can calculate upon the time when it may be expected. In remittent fever, especially during the first days, there is usually a distinct moisture appearing a short time after the febrile paroxysm begins to abate.

7. The tongue at the onset of typhoid fever is usually little different from its healthy state; it gradually becomes red at the tip and sides, a whitish fur forms on the upper surface, through which red papillæ show themselves. As the disease advances it becomes dry, occasionally casting off the fur, and assuming a smooth shining appearance; frequently it has a dark brown coat. In bilious fever, the tongue is usually covered with a whitish fur at the commencement, if not, it becomes so speedily, the fur changing to a tawny or yellow, and in bad cases to dark brown, when it likewise becomes dry. When the tongue begins to cast off its fur, it is done gradually, commencing at the tip, and is always hailed as an evidence that improvement has commenced; the part from which the fur has separated looks well, neither unnaturally red nor dry.

8. The stomach is rarely much troubled with nausea or vomiting in typhoid fever. In bilious fever, gastric derangement with vomiting of bile is a frequent symptom.

9. In typhoid fever diarrhea, more or less considerable, generally attends from the commencement of the attack, frequently it is observed to have existed prior to it. The dejections are different in color and consistence; in a ma-

jority of instances not showing a very marked alteration in the quality or quantity of bile, as compared with stools of similar consistence, occurring in persons otherwise healthy; but whatever the color, they are attended by a horrible fetor, which has been termed cadaveric. In bilious fever, there is generally a decided tendency to costiveness, which usually remains more or less marked during the attack. When stools are procured by purgatives they many times have a very offensive odor, but the odor is of a bilious, not of a cadaveric character. It is true that in some epidemics of bilious fever, there is considerable *bilious diarrhea*, the dejections being so acrid as to excoriate the anus.

10. There would seem to be more epigastric tenderness in typhoid fever; there is also a notable tenderness in the right iliac region which I have not observed in bilious fever. Tympanites is almost always present to a greater or less degree in typhoid fever; not commonly so in bilious fever.

11. It would seem from the observations of others, (not from my own,) that rose colored lenticular spots on the skin are very generally to be found in typhoid fever. I have found sudamina present in something like a third of the cases in that disease. I have also seen desquamation of the cuticle in about the same proportion. I have had a good deal to do with bilious fever, and am not aware that there is any peculiar eruption attending it. It is true that occasionally an eruption appears in consequence of irritation of the primæ viæ; but such eruption varies very much in its appearance. Whelks as large as the finger nail, red, decidedly elevated and attended with a distressing itching; and numerous vibices, red and slightly elevated, have been perhaps the most frequent. The rose spots, desquamation of the skin or sudamina I have not seen. When an eruption appears at all (unless it is petechial) it is commonly at the commencement.

12. The duration of typhoid fever upon an average, may be put down at three or four weeks. Unless in some cases which appeared to be this fever, and which were relieved in a few days, I do not know that I have seen a case decidedly convalescent before the tenth day. I should say that the average duration of bilious fever is not more than two weeks, if that long. Many cases are cured in three or four days, and the ninth day is popularly considered *the* critical day. Either disease may extend to sixty days or more.

13. Another distinguishing mark may be stated, to-wit: the effects of remedies. In typhoid fever general *bloodletting* rarely is called for, and in my observation when used, has been followed by no benefit. In bilious fever, it is frequently of much advantage; directly, in allaying febrile excitement; indirectly, in favoring the operation of cathartics. *Emetics* need to be watched in typhoid fever to prevent their increasing the diarrhea. In bilious fever, they do not produce diarrhea, on the contrary, we frequently give a purgative on the same evening after an emetic—indeed not unfrequently combine them. *Cathartics*, particularly calomel, operate differently in the two diseases. In typhoid fever, a moderate dose is beneficial at the beginning. it may produce bilious purging, but almost always the color is yellow, and the consistence thin; neither can we make much alteration in the quality of the dejections, at least for the better, by increasing the dose. In bilious fever calomel is emphatically *the* purgative upon which chief reliance is to be placed. A moderate dose, say five to ten grains, may cause watery purging, but increasing the dose to fifteen, twenty, or thirty grains changes the character of the dejections very much. They now become consistent, bilious, and dark bottle green in color. Such stools in bilious fever almost universally, (if I were to say universally I think I should tell the truth according to my observation,) are followed by amend-

ment. Such stools in typhoid fever I have never seen from any sized dose of calomel, and I have given it in doses of five to sixty grains.

In some of the cases of typhoid fever in which the stools approximated nearest to that character, the disease continued to grow worse and eventually proved fatal.

14. Typhoid fever does not admit of much activity of treatment. Bilious fever requires considerable activity.

It will not be supposed that the foregoing remarks apply to extreme or irregular cases of disease, but the usual form and as generally applicable to the first week of disease, (unless a later period is obviously referred to,) when we may be presumed to be required to identify the disease.

In the latter stage of disease, that which heretofore has been called the *typhoid stage* of disease, the prominent symptoms will be very much the same, whether that disease be a fever properly so called, (bilious, typhoid, typhus,) an exanthema, on a phlegmasia. So we will say nothing as to that stage.

Since writing this diagnosis I have examined Professor Wood's work on Practice in which he expresses the same opinion as to the difficulty of diagnosis in some cases between typhoid and bilious fevers. "I have seen a case of this kind in consultation, which was considered by a skilful and experienced physician as bilious fever, but which I suspected to be enteric fever, from the state of the bowels, connected with commencing meteorism and the character of the cerebral symptoms. The patient died in about a week, and glandular patches of Peyer were found enormously enlarged," v. 1, p 323. Again, "I believe that occasionally the two complaints are commingled, in consequence of the co-operation of their causes. Thus cases having all the essential characters of enteric fever occasionally end in intermittent; and bilious fevers or

affections which cannot be distinguished from them, sometimes show the signs of enteric fever during their progress." p. 329.

ARTICLE 3.

THE DIAGNOSIS OF TYPHOID FROM TYPHUS FEVER

Is held by those who consider these as distinct diseases to be a matter of no great difficulty. Absence of diarrhea, of tympanites, of tenderness of the abdomen; presence of petechiæ are the marks of typhus. Diarrhea, tympanites, tenderness of the abdomen, gurgling upon pressure in the right iliac region, rose spots, are the diagnostic marks of typhoid.

In order to put this matter in a clear light and show the strength of conviction abiding with many as to the distinction between the two diseases, I will insert a letter from Dr. Power, of Baltimore, to Dr. Bartlett, found at page 272 of his Treatise on Fevers. "The questions you ask have interested me very deeply, and this summer for the first time, I have had ample opportunity to fix definitely my own opinions. What I write you now resumes the opinions also of Dr. Chew and T. Buckler, who saw the disease throughout its whole visitation in this city; nor do I know of one gentleman who had any opportunity of studying it here who differs from us.

"We have had for the last fourteen months an epidemic of typhoid fever in Baltimore. The wards of the almshouse and infirmary have constantly contained a large number of cases of this disease, presenting nothing remarkable, save that the cases had, as a general rule, more the adynamic type than in former years, and required more stimulation. Early in May two vessels arrived

bringing Irish emigrants, one from Liverpool, the other from the South of Ireland. Other vessels succeeded these, so that upwards of two hundred cases were treated at the infirmary, and upwards of eighty at the almshouse. These cases were *typhus* so exactly corresponding with Gerhard's description of the Philadelphia epidemic of 1836, that I am constrained to say, I know of no better portraiture of any disease, than that which he has given of typhus fever. I made or assisted at twenty-six post-mortem examinations, in *not one* did I see any trace of the peculiar lesion. In nine of those who died having loose bowels during life, we found either the lesions of dysentery or of diffused muco-enteritis; no mesenteric alteration in any case. The parenchymatous organs and mesenteric vessels were congested with dark fluid blood; and the condition of the spleen, bronchial mucous membrane, lungs and brain resembled what we find in congestive remittent or typoid cases. The stomach was uniformly more altered, and presented deeper traces of inflammation than in typhoid fever. We had fourteen autopsies of typhoid fever during the same period, and it thrice occurred that we had the bodies opened side by side for the sake of comparison made at the time.

"The mode of access, facies, march, eruption, symptoms, treatment, and convalescence are all different between the two diseases. We had both forms of fever at once under our observation, German emigrants and domestic patients, with typhoid; and Irish and English with typhus. Nay, more than this, four of the seamen of the *Rio Grande*, a vessel which brought seventy cases of typhus, had true typhoid fever, and several of the steerage passengers had the same disease. There was no mistake in the diagnosis in any case where the disease was fatal, as proved by the autopsy; and in the successful cases, the difference of the eruption, diarrhea, and meteorism, the peculiar nervous symptoms, the great emaciation, bed

sores which were so rare in the worst cases of typhus, that I saw but two, made the diagnosis simple to every clever student. The effect of full stimulant treatment made the difference still more obvious. In short we have here in Baltimore *no doubt*, but the fullest conviction of the non-identity of the two diseases.

"Furthermore, there is the undoubted contagiousness of typhus. Two of the Sisters of Charity at the infirmary—one of whom died—and three out of five resident students took the disease. Four of the hospital assistants, and several of the inmates of the almshouse were attacked; many cases occurred also in the city, where direct intercourse could be detected and proved.

"Here is another interesting order of facts: a German had *typhoid fever* and was eight days in bed under Dr. Buckler, he recovered, and was acting as hospital assistant; in tending the sick immigrants he was seized with *typhus*. Two years ago, Dr. Berryman had, at the almshouse a severe attack of *typhoid fever*. He was appointed resident physician at the new quarantine hospital, where all cases are now sent. He took the fever and died last week—the most promising young man I have ever known, and whose loss has filled us all with grief. Again, one of the immigrants who came near dying in May last with *typhus*, is at this very moment at the point of death with *typhoid fever*.

"There is as much difference in my mind between the two diseases as there is between measles and scarlatina. Huxham has beautifully drawn the distinction between the slow, nervous, and malignant fevers. Excepting the new lights we have in pathology, we can add but little to what he has said. Corrigan, in Dublin, sees the difference between what he calls *enteric* and *typhus fevers*. Dr. Wood, in his late work, appears to me to have handled this subject better than any other of our systematic writers. I perfectly agree with him in his conclusions. It

appears to me that we are better placed than either the French or English to study this question without prejudice, and more likely to arrive at the truth." Drs. Gerhard and Wood, of Philadelphia, are equally confident of the correctness of this view of the subject.

If this subject could be settled by the authority of a very considerable number of highly eminent men, it might be now considered as put at rest. But whilst expressing my unfeigned and very high respect for, and confidence in these gentlemen, I am not yet prepared to consider the matter as placed beyond controversy. They prove conclusively that according to their observations, a certain train of symptoms has almost always, (always if you please,) been followed by ulceration of Peyers' plates—that after another train of symptoms, these plates have never been found ulcerated. Hence, they conclude that these two trains of symptoms denote two diseases specifically different. They may be right, but are they necessarily so? According to my observation, and that of physicians immediately in this vicinity, the rose spot does not appear as a symptom of typhoid fever. Am I to set this against the observation of the profession, and conclude that this eruption is not characteristic of the fever? Assuredly not. But the symptoms of typhoid and typhus fever do not differ more than those which attend different epidemics of bilious fever. For example, we may have in one summer, an eruption of red pimples or blotches, great nausea and vomiting, and bilious diarrhea to attend it. Perhaps the next summer it will be without either, and obstinate costiveness be present. Yet we hold the two years to have produced the same disease, very much modified, it is true, in its appearance. Again, is there more difference between the symptoms of typhus and tyyhoid fevers, than between those of the ordinary intermittent, and aggravated forms of bilious fever of the re-

mittent or congestive type? Yet we consider them essentially the same diseases.

Dr. Power informs us that the vessel Rio Grande brought seventy cases of *typhus*, and that four of the seamen and several steerage passengers had *typhoid* fever. Professor Drake found among the immigrants at Gross Island, patients with typhus and typhoid fever: i. e. those with rose spots and those with petechiæ—a rather equivocal evidences of the non-identity of the two diseases; yet certainly not proving their identity. But we find others as well situated to observe, and as well qualified to judge, view the matter in a light very different.

Thus M. H. Landouzy gives in the *Archives Generales de Medecine* for January and February, 1842, an account of an epidemic fever which prevailed at Rheims in 1839 and 1840. If it were possible to identify a disease by symptoms, it would seem that this was unequivocally one of typhus fever. A copious extract of an account of this epidemic, which is too long to quote, is to be found in Bartlett's work on typhoid and typhus fevers, p. 302 *et seq*: "In the six autopsies which were made, the intestinal lesions characteristic of typhoid fever were present. The elliptical plates were either thickened and ulcerated, or they were the seats of ulcerations, and the mesenteric glands corresponding to them were enlarged." In view of this state of things—the symptoms of typhus and lesions of typhoid—Professor Bartlett asks: "Is it possible, that even admitting the two diseases to be essentially dissimilar, under certain circumstances, the causes of both may be so commingled as to give rise to a mixed disease, in which there is a mixture of the elements of both?" *loc cit.* Professor Wood gives into this idea of the occasional amalgamation of the two diseases. Thus, vol. 1, p. 346 of his Practice, he says: "The peculiar disease of the glands of Peyer is never found in typhus, or so seldom as to lead to the suspicion of some intermixture

of the diseases, when it does occur." Again—"Nevertheless there are cases which cannot be clearly distinguished; and, as before suggested, it is highly probable that the two diseases now and then exist conjointly together" *Id.*

"Gerhard acknowledges to have met with six cases of true typhus in which the intestinal lesion resembled dothinenterite, and admits that the follicular ulceration occurs in the British epidemic, which he regards as true typhus, but says that it is merely accidental—a complication, not in the ordinary course of the disease." *Dickson's Practice*, v. 1, p. 412. With regard to the amalgation of specific diseases thus suggested by Profs. Wood and Bartlett, it would certainly ill become me to deny the possibility; but inasmuch as the explanation seems to me gratuitous, and so far as I know unsupported by analogy, but rather contradicted by it, I think it ought to be entertained with caution and hesitation. Under such admissions by Bartlett, Wood, and Gerhard, I do not know that it would be too much to say, in the words of *Gualtier de Claubry*, that "there are no means of distinguishing typhus from typhoid fever, in relation either to lesions or the symptoms of the two diseases." Again: "The identity of the two diseases is henceforth put beyond a doubt." *Bartlett*, p. 302.

Of myself, I ought here to say that I know nothing of typhus fever if it differs from typhoid, as I have never seen a case; and all I have said is based upon the writings of others.

ARTICLE 4.

NATURE.

SEC. 1. *Rose Spots.*—What is the nature of this affection? Is it an idiopathic, contagious, eruptive, self-limited fever?

Or is it a variety of typhus fever? Or is it an element which may enter and assist in making up various diseases? The weight of authority seems at present to be, that it is an eruptive, essential, self-limited fever, whose presence is determined, at least in fatal cases, by the state of the elliptical patches of the lower portion of the ileum. One of the most common and characteristic symptoms being a rose colored eruption on the skin, showing itself during the second week of the disease. It is true, it is not asserted that this eruption shows itself in every case. Louis detected it in forty-nine of fifty-four cases, or seven in eight. Bartlett says that it is present in nine-tenths of the cases. On the contrary, 'of seventy cases referred to by Chomel and Genest, they were absent in sixteen, or about one-fourth'—Wood, v. 1. p. 320. That this is true of the disease as observed by these gentlemen, I am perfectly satisfied; but I am as fully satisfied that such is not true of it as it has appeared in this neighborhood for the last six years. I am assured of this, because I have been much interested in investigating the disease. As before stated, I have examined patients daily during the whole course of disease without finding a single spot: I will add, too, that I have not confined my examinations to a few patients or to a short period. It is my custom to examine all whites. I have also inquired of many of my medical friends, and Dr. Evans, as before noted. is the only one who has seen the eruption, unless Dr. Gano may be an exception. The result of his recollection is given in the former part of this essay.

These facts lead me to doubt whether this eruption is peculiar to this disease—an essential symptom. It may be alleged that fevers purely eruptive, sometimes run their course without their characteristic eruptions making their appearance. This may be true. But is it true that any eruptive fever ever prevailed, in which one-fourth or even one-tenth of all those affected failed to exhibit the

eruption? Or is there any instance in which an eruptive fever prevailed for six years in any one location, without the eruption being observed, though diligently sought for? One of the cases, the dissections of which are detailed in this essay, was a white man, who had been subjected to daily inspection by myself during my attendance, which commenced on the fourth day. The eruption was certainly absent in this case; but the intestinal lesion was present. The one then may appear without being preceded by the other. The time too, at which the eruption ought to have appeared had passed before death. Again, sudamina, which are represented as appearing later than rose spots, did appear. The other cases were negroes, from which nothing can be inferred, as I am not aware that the eruption has ever been detected in one of that race.

The space of time during which this eruption makes its first appearance is rather too long for a specific eruption—that is, if we are to reason on this disease from analogy. This period is stated by Louis (on Fever, v. 2, p. 198, Bowditch's editin) to be from the sixth to the thirty-fifth day [thirty-third, B.] according to my computation it should have been the twenty-fourth day. See v. 1, p. 93. Without laying any undue stress upon this, as the observations upon this point, at least so far as I am acquainted with them, are not sufficiently numerous and precise to be of much value, I would remark, that I do not know of any exanthem in which so much latitude in the period of eruption is allowed, there being nothing to interfere with its regular development. By the by, is there an oversight or misprint in the following sentence? 'But there is good ground, I think, for believing that those bounds [of the range of typhoid fever] are wider than those which circumscribe the prevalence of any other strictly idiopathic non-eruptive fever.' Bartlett, p. 85.

I certainly have been under the impression that Prof. B. considered this disease an exanthem.

But as the eruptions are considered as of much importance in distinguishing typhoid from typhus fever, they ought themselves to be distinguishable. Let us examine a lot of cases investigated by Dr. Shattuck, with particular reference to this subject. Of six cases given by him, the 3d, 5th, and 6th exhibited both eruptions. *Bartlett*, p. 294. We will also hear Dr. Stewart upon the eruption of typhus fever. Prof. Bartlett tells us, *op. cit.* p. 210, "Dr. Stewart, amongst others, has within a few years studied with great care and particularity the characteristic appearances of this eruption. He says that the rash is permanent; that is, that it does not consist of a successive eruption of spots; that in all cases it presents the two periods, longer or shorter, of increase and decline; and that in the more severe cases it may exhibit, during the period of increase, four different states, being florid, dark, livid, and petechial. When the hue of the eruption is florid, it disappears readily under the finger: when dark, it still disappears, but more slowly: when livid, semi-petechial or pseudo petechial, as it has been called, it is only partially effaced: and when petechial it is not at all affected by pressure. In many cases it remains florid throughout; in others it presents one or more, and in not a few all these alterations: and after it has reached its height, the process is inverted, and it passes through the various phases of lividity, darkness, redness, and paleness before evanescence. This change from rose spot to petechia was also observed by Prof. Drake, at Gross Island. *West. Journ, Med. and Surg.*, Oct. 1847. *Bost. Med. and Surg. Journ.*, v. 37, No. 8. Also by Dr. Upham, at Deer Island Hospital. *Bost. Med. and Surg. Jour.*, v. 37, Nos. 25 and 26, and early Nos of v. 38.

Respecting this eruption, there is another point not altogether unworthy of consideration. Prof. Bartlett re-

fers, p. 108, to several cases in which this rose colored eruption appeared during a relapse after an attack of this fever* This, it seems to me, is proving too much. I do not know that the eruption peculiar to small pox, measles, or any other exanthem has been observed to reappear after having disappeared in the regular course of disease. If my notion on this point is correct, reasoning by analogy, we should not expect the eruption of typhoid fever to reappear, considering it an eruptive disease. If, then, the eruption in the second instance is accidental—not proper to the disease, the same eruption appearing in the first instance, may be accidental—not essential to the disease, but dependent on intestinal irritation.

Here I feel disposed to refer to a case of scarlet fever which occurred to me in December, 1838, as being somewhat analogous to the account given above from Stewart.

"Helen Johnson, aged 2 years, has had no opportunity of contracting scarlet fever, except that her mother has visited a family affected with it—was taken on the 17th December, with an eruption in distinct, elevated acuminated points, not colored; tongue whitish with red papillæ; heat moderate, and pulse as is common in ordinary mild fever. 19th, much the same, except that there has been considerable nausea and some vomiting; tongue cleaning off at the tip and edges, rather smooth, but neither red nor dry. 21st. Has been quite lively since last report; ate heartily yesterday. At present feet very much swelled; eruption on feet and legs distinct, red, from one to three lines in diameter; also a considerable patch on the thigh; tongue clean, smooth, papillæ distinct but not red, moist and nearly natural in color; considerable hoarseness and soreness of flesh. 22d. Feet less swelled, eruption on feet, legs, and thigh purplish and fading, tongue

* A similar case is noticed in this Essay.

as yesterday, child lively. 28th. The red blotches are still apparent on various parts of the body; appetite good, tongue assuming the natural appearance." I think it not a little singular that the mother of this child sickened on the 20th of same month, had the same elevated acuminated colorless eruption upon the hands and arms; the same red spots upon the body; had the skin exfoliating on the 28th, that of the hands coming off in patches of two or three inches in diameter. I will add that this lady died in 1841, of perforation of the bowel, occuring in typhoid fever."

After considering maturely the above extracts from Stewart; after noticing the observations of Drake, at Gross Island, and Upham, at Deer Island; after having seen a case apparently parallel in scarlet fever, is it unfair to doubt the distinction between the rose spot and petechia? If the same identical spot is a rose spot one day, a petechia the second, and a rose spot again on the third, it does seem to me that distinguishing them apart is alike impossible and useless. Hence we must not be surprised—nay, we may expect that the rose spot and petechia shall eventually turn out to be the same eruption under different modifications.

Sec. 2. *Sudamina.*—Louis found sudamina present in about two-thirds of his cases. This eruption is said to be found more frequently in this than in any other acute disease. It has been found in a large proportion of cases in this vicinity. Here, then, is a second cutaneous eruption, more or less characteristic of typhoid fever. Indeed Dr. Hale, of Boston, who had good opportunity of studying the disease, enumerates both the rose spot and sudamina as among 'the distinctive marks of typhoid fever." *Am. Jour. Med. Sci*, v. 25, p. 396. I do not know of any other exanthem being allowed more than one cutaneous eruption. Is typhoid fever a disease which bids defiance to analogy?

SEC. 3. *Peyer's Patches.* We come now to the intestinal affection—the dothinenteritis, which I think those who place this fever among the exanthemata ought to consider the characteristic eruption. It is held to be characteristic of the disease—essential to it—that it is found in no other acute disease, if in any at all. That this affection of the intestinal mucous membrane is to be found in this region, dissections have sufficiently shown. It has been found, too, in cases which exhibited a fair proportion of symptoms marking typhoid fever. But whether that condition is the characteristic mark of a specific disease, let us now inquire.

We find that Dr. Lombard, of Geneva, who appears to have been familiar with typhoid fever, both as to its symptoms and pathology, for six years in Germany, upon visiting Great Britain saw cases which he declared could not fail to exhibit ulcerations which he considered essential to that disease. Upon examination of the bodies, however, he was astonished to find no lesions such as he expected.—*Amer. Jour. Med. Sci.*, v. 26, p. 400. *Bartlett*, 257. Dr. Shattuck, of Boston, after studying typhoid fever in Paris under Louis, at his request, examined cases of fever in England, with a view of determining the identity or non-identity of the fever of the two countries. He concludes that typhoid and typhus fevers are distinct, though both prevailing at the same time. Yet he gives us a case of what he considers typhoid fever, which appears indeed to have been as well marked as most cases of the disease; but "there was no appreciable alteration of the elliptical plates or of the mesenteric glands.—*Bartlett*, 294. Fouquier reports a case in which the symptomatology of typhoid fever seems to have been pretty clearly marked, and in which the elliptical plates and mesenteric glands were found almost free from disease.—*Id.*, 116. Prosper Dor reports a case, of which Professor Bartlett says: "Certainly in this case the diagnosis

during life would have been sufficiently clear and positive." Yet, "examination after death showed the intestines healthy."—*Id.*, 117.

Having briefly referred to some cases in which the symptoms would have seemed to point out ulcerations, or at least serious disease of the elliptical plates which nevertheless failed to exhibit that lesion upon examination, I will now turn to the converse proposition. Here it will not be amiss to make an extract from Professor Bartlett's work. "In all cases of typhoid fever there is lesion of the small intestines. This lesion is peculiar. It is found in no other disease," p. 58. Of this "invariable and characteristic lesion," I cannot give a better or perhaps a more succinct description than is to be found in the work referred to. "In a small proportion of cases, consisting of those which terminate early, the elliptical plates together with the subcellular muscular tissue are merely increased in thickness, with redness and softening. This increase of thickness is such that the edges of the plates project from one to two or three lines above the surrounding mucous membrane. Sometimes the hypertrophy of the plates and of the subjacent cellular tissue is quite simple, the color and consistence of the membrane remaining unaltered. This simplest form of the lesion, like all others, which are more complete, is universally found most advanced and most strongly marked at the lower extremity of the ileum. Each successive plate as we go upward along the intestinal track from the ileo-cecal valve, is less profoundly altered, till we arrive at those in the natural condition. The number of plates thus changed is very various. [From one or two to thirty or forty.] The surface of the thickened plates frequently presents a granular or finely mamelonated appearance occasioned by an enlargement of the gray orifices of the cryptæ which go to make up the plates. This condition becomes very

manifest when the gland is detached from its subjacent tissue and held between the eye and the light. At other times the surface of the thickened membrane corresponding to the plates is quite smooth and level.

"In a great majority of cases, the plates, instead of being merely thickened with or without redness and softening, are more or less extensively the seat of ulcerations. These ulcerations vary very much in size and number. It frequently happens, for instance, that in proceeding from above downwards, in our examination, after having passed over several plates simply thickened, we come to one of them in which there is a single circumscribed ulceration with perpendicular edges, extending more or less deeply into the thickened tissues. As we go on towards the termination of the intestine, the ulcerations become more and more numerous and extensive, till at last for several inches next the valve the plates are entirely destroyed, and we find only ulcerations corresponding to their sizes and shapes occupying their places.

"These intestinal ulcerations are commonly more or less rounded or oval in their shape. Sometimes, however, their borders are irregularly jagged and angular. So their edges are in most instances pretty regularly perpendicular and smooth, but sometimes they are ragged and shreddy. The bottoms of the ulcerations vary of course with their depths. They consist sometimes of the cellular tissue immediately under the mucous membrane; sometimes of the muscular coat; and sometimes of the peritoneal covering. Occasionally this covering itself gives way, perforation takes place, and the contents of the intestine are discharged into the cavity of the peritoneum."

Keeping this description in mind—which indeed I think very excellent, (if we supply a small omission, viz.: that the membrane between these thickened or ulcerated plates, as the case may be, is frequently more or less highly injected, and traversed by enlarged veins filled

with dark blood,) let us see whether this state of things exists in no other disease.

Broussais' Chronic Phlegmasiae, vol. 2, p. 106, gives a case of intermittent fever changed into continued fever; death on the sixty-ninth day; in which among other appearances observed upon dissection, we have: "The mucous membrane of the small intestines in its upper part presented some isolated red points; lower down near the end of the ileum it was of a deep red, granular, black, and generally sphacelated and ulcerated. An analogous disposition throughout the whole length of the colon. All the granulations were so many small ulcers with loss of substance of the membrane; the appendages of the intestine studded with small black glands."

It may be objected that this, and some other examples cited in this essay are not the ulcerations marking typhoid fever—that these last consist of inflammation and ulcerations of the solitary and Peyer's glands—that such is the legitimate inference is true—that such is more than intimated by Professor Bartlett is likewise true. He says after Chomel: "That one of the most constant and uniform characteristics of secondary lesions, consisting generally of specific inflammations, is the fact of their being *disseminated;* of their occupying numerous and circumscribed spots in the tissues and organs of the system," p. 142. Again: "Now in every respect, the intestinal lesion of typhoid fever corresponds to this class of pathological alterations. It is disseminated; occupying the same glandular tissue at different points of the intestinal mucous surface," &c., p. 144.

It is true he does not assert that the ulcerations are confined to the glandular structure and the mucous membrane covering it; but such is clearly the impression conveyed by the language. If this interpretation is the correct one, the objection above supposed is valid. But that such is not always the fact I have evidence to me conclusive. In Sylla, the autopsy of whom is given in this

essay, many of Peyer's patches and the solitary glands were ulcerated; and for aught I know, the ulcerations may have been confined to the bounds of the glands; but at the lower end of the ileum there were several very irregular ulcerations, one of which was two inches long, by one and three quarters wide; a tongue of membrane not ulcerated, running up the broad end of the ulcer about half an inch wide and three quarters long. In the cecum and colon were similar irregular ulcerations. I hold that these ulcerations were not situated in Peyer's plates at all, because the form and proportions were not such as to admit that interpretation. Or if they were so situated, the contiguous membrane must have been involved. I had the pleasure of showing the casts of some of these ulcerated portions of intestine to Prof. Bartlett, who recognised them as examples of the lesions of typhoid fever.

In *Horner's Path. Anat.*, p. 307: "In a patient dead of phthisis pulmonalis, it is said the jejunum and ileum abounding in large ulcers of the mucus coat, which went around the circuit of the gut and were twelve or eighteen lines wide. * * * The beginning of the colon, on internal face, covered for some five or six inches by ulcerations, also the adjoining part of the ileum." Again, of a patient dead of tubercular consumption, supervening upon epilepsy it is said: "The glands of the mesentery were enlarged. * * * Numerous patches of red inflammation, looking like the precursors of ulcers existed in the mucous coat of the intestines. There were several ulcers from two to five lines in diameter, in the small intestines, and a few in the large," p. 370.

In Wistar's Anatomy, by Pancoast, these glands are represented as very frequently the seat of lesion in phthisis as well as in typhoid fever; no attempt to discriminate between them, being made.

In connection with these alterations as observed in phthisis, I will introduce an account of a case which I saw last spring, (1847,) which I consider of some value.

Matthew, aged twenty-two, until a year ago remarkably stout and healthy. Then became affected with severe cold in consequence of exposure. He continued more readily influenced by fatigue than common, yet not to such a degree as to attract marked attention. About Christmas, he was seized with hemorrhage from the lungs, he coughed much, and was disabled from work. He, however, improved afterwards, and thought himself able to break hemp. He was soon obliged to desist on account of recurrence of hemorrhage. March 5.—I saw him rather accidentally; at which time his pulse was ninety; frequent hacking cough, but no blood expectorated; skin ashy and dry; tongue furred white; muscular weakness; respiration frequent and easily hurried; considerable emaciation. He took about this time, some doses of calomel, opium, and ipecac; and subsequently some small doses of ipecac and tar water. He seemed to improve for a time, and about the first of April drove an ox-cart to town, a distance of five miles, without much fatigue. He then appeared better, but his pulse remained frequent. On the 13th of May I was desired to see him, as he was thought to have typhoid fever, and his master thought that, possibly if he could survive, the attack might effect an improvement in his condition. I found him covered with a profuse perspiration, tongue furred white; cough as before, but discharging considerable quantities of pus; respiration very quick, considerable dullness on the right side of chest, left tolerably clear; no diarrhea, but some tympanites; pulse, sitting, one hundred and thirty-five; lying, one hundred and twenty; slightly delirious. I gave him small doses of blue mass and Dover's powder, directed attention to bowels and general comfort; and informed his master that I saw nothing but the progress of phthisis, that—I thought the case hopeless. I did not see him again. June 2.—His master informed me that he had died that morning,

and as I had expressed a wish to examine his body, I could do so. He also told me that Matthew had continued to grow weaker from the time I saw him. His pulse smaller; his mind more unsteady; until a few days before death he was compelled to lie in bed altogether—having considerable trembling of the hands—pulse too frequent and weak to be counted in his tremulous condition. He had continued to cough, but about a week before death ceased to spit pus—had complained of no pain or tenderness in abdomen—had continued costive, for which a laxative had been administered every alternate day until the last four days of his life, during which he had no stool.

Autopsy seven hours after death.—Emaciation very considerable but not extreme. Abdomen not distended. *Thorax.*—Heart natural; left lung generally healthy, but a few tubercular masses interspersed through its substance; right lung a complete mass of tubercular aggregations, with the intervening parenchymatous tissue condensed and reddened; the whole lung attached to the pleura-costalis by adhesions apparently not of very long standing. *Abdomen.*—Stomach apparently healthy externally, not opened; duodenum of good appearance—upper part of jejunum of same description; the lower portion and ileum dark and contracted in diameter, except in small portions here and there, which were of healthy color and dimensions. Along this range at distances of from six inches to a foot, were patches distinctly thickened, as observed by feeling on the outside; upon opening the bowel, they were found exalted in color, projecting about a line above the surrounding surface with minute ulcerations on the surface. These patches were fifteen to eighteen lines long, by eight or ten wide. Many solitary glands were ulcerated. In the last two feet of the ileum were only two of these patches; above that, they were frequent. The appendicula vermiformis was

very dark, which color was communicated to that portion of the peritoneum with which it was in contact. There was also a small collection of pus at this point. *Colon* much distended except for two or three inches near the commencement of the descending portion, where it was of less calibre than natural. Upon laying open this portion there was considerable thickening of the mucous coat with slight discoloration, also an ulceration of a line in diameter. Here was a minute particle of well elaborated feces, such as are frequently found in the watery stools of typhoid fever, about the time amendment commences; it was perhaps a fourth of an inch long by a sixteenth. Otherwise the bowels were more completely empty than I ever saw them, or indeed imagined it possible they could become. It was only in portions that the mucous membrane was even tinged with apparently healthy bile. In various other portions of the colon there were spots of discoloration and evident thickening. *Liver.*—On convex surface healthy, on concave rather dark, but apparently healthy in structure. Gall-bladder filled with bile. *Pancreas and kindneys* healthy. *Spleen* nearly globular, dark claret color, with numerous tubercular masses on surface; upon being laid open, it was very thickly studded with tubercular masses, globular, of the size of a small rifle bullet. *Mesenteric glands* slightly if at all enlarged; the largest of the size of a small filbert; it was not colored, and being cut into presented several minute points of tubercular matter. Behind the root of the mesentery were several masses of tubercular deposit. To put this matter in as clear a light, in all its bearings, as possible, I add that when I first saw this boy in March, he was in the room with his brother laboring under typhoid fever.

Was this a case of unmixed phthisis or of typhoid fever supervening on phthisis? Are there any cases on record in which typhoid fever came on in the last stage of phthisis?.

Recently, November, 1847, I saw a case of fever in a patient who was thought to have been afflicted with consumption for some time. I could not detect any pectoriloquism, but some dullness of chest. Although very anxious to make an examination after death, I was not allowed the privilege.

For the purpose of acquiring the most satisfactory information upon the condition of Peyer's plates in typoid fever, as compared with small pox, measles, &c., I requested information of Dr. Holmes, Professor of Anatomy, and Dr. J. B. S. Jackson, Professor of Pathological Anatomy, Boston. I applied to these gentlemen because I believed that this subject has been more thoroughly investigated in New England than in any other part of the United States. Professor Holmes says: "I am not aware that ulceration of Peyer's patches are met with in measles or small pox. The mark of ulceration in phthisis consists in the presence of tubercle; but if none of this substance be present there may be a great deal of doubt—as in a case which I saw some months since in South Boston." Professor Jackson informs me that he has often seen Peyer's patches red in measles, scarlatina, pneumonia, and croup, but never ulcerated. He has never examined them in small pox, and is not aware that they have been found ulcerated in that disease. These remarks apply to observations made at periods when in cases of typhoid fever, ulceration would have been expected to be present. In phthisis he does not think he has seen ulceration of the patches without more or less tubercular deposit about them, though such ulcerations are, he believes, generally found in the large intestines.

I not long since held a conversation with a distinguished gentleman; during which he remarked, that an expert pathological anatomist would readily detect the difference between the ulcerations in phthisis and those of typhoid fever; but he did not feel himself competent to do so with

certainty. He mentioned the state of the spleen and mesenteric glands, as being sufficient to determine the point. My reflection at the time was—if you cannot determine the point, I guess its determination must be a matter of conjecture rather than of demonstration; and involuntarily thought of Professor Potter and the measles. Let it be remembered that we are considering this lesion as to its peculiarity in typhoid fever—as being of itself sufficient to demonstrate the existence of that disease, not as it is interpreted by previous history of the case or concomitant lesions of other organs.

Professor Bartlett *op. cit.* p. 119 says: "There is no good reason to think that positive ulceration of the intestinal follicles usually takes place in mild cases, and perhaps not in many of moderate severity." "It is certain that numbers of patches are frequently found without ulceration—that there is a time in every case when none are ulcerated. Sudden congestion or other complication might supervene at this time and cause death." In what would the patches in these causes differ from their appearance in measles, scarlatina, or croup. Again, "There is another peculiar appearance of the diseased plates, which is found in a certain proportion of cases; according to Louis, in somewhat less than one-third. This seems to consist in a morbid change or transformation of the subcutaneous cellular tissue. Instead of being simply hypertrophied, with or without redness or softening, as in the cases already described, this tissue is converted into a substance of a yellowish color, destitute of any traces of organization, presenting a substance somewhat glossy when cut, and about as hard and friable as crude tubercle." How are these cases, which according to Louis amount to nearly one-third of the whole number, to be discriminated from those of phthisis, if we look to these plates alone for the distinguishing features? It does seem to me, that if the description above given, is not

one of tubercular matter, it is one which it is very difficult to discriminate by words from it.

There are two points concerning this lesion which it may be proper to mention, more as a means of keeping the course of the disease before the mind, than in any way elucidating its pathology.

1st. That the disease may terminate fatally, notwithstanding some considerable progress may have been made towards restoring the healthy condition of these patches. Thus, we see in Louis, v. 2, p. 166, in a patient dying upon the twenty-first day, some progress in restoration had taken place. This had been a case of considerable severity. Again, at p. 62 we have another autopsy of a patient dying on the twenty-ninth day evincing the same state of progressive improvement, as evenced by a grey, bluish tint in the parts ulcerated, and the mucous membrane generally. Again, v. 1, p. 122, we have a considerable progress towards healing in a patient dying on the forty-third day. One remark in passing: about the twentieth day, according to our present information, seems to be the earliest period at which the process of restoration commences. Here we have death occurring at that period notwithstandidg the appearance of healthy action was visible. I do not think that it is common in eruptive fevers, for the patient to die after the eruption, having progressed regularly, has begun to decline at the shortest period. I have seen cases of typhoid fever, it is true, in which congestion of the lungs or other secondary symptoms supervene suddenly and carried off the patient, he seeming to die, not of the fever but of congestion. But such does not seem to have been the fact in the cases referred to.

2d. There seems to be very great lattitude as to the time this cicatrization commences. We have seen that it may have made some progress on the twenty-first day. On the other hand we have a case, *op. cit.* v. 1, p. 130, of

an apparently decided convalescence, setting in on the twenty-first day, continuing for eighteen days; a relapse and death on the sixty-fifth day, in which no progress towards restoration in these patches had been made. It would thus seem that there is no certain relation between the period of disease and this lesion; nor between the healing of this lesion, and the life or death of the patient. It may however be said that improvement may have commenced in these patches at the time convalescence appeared to begin. To this there are two objections: 1. In one of the cases quoted, healing had commenced although the case was going on to a fatal termination. 2d. In relapses in eruptive diseases, the eruption does not reappear.

Describing the pathological appearances in patients dead of infantile remittent fever, Mackintosh says: "The ulcerations in the ileum and colon, strictly resemble those which I have afterwards to describe in the bowel complaints of children, except that the whole mucous surface of the colon is occasionally involved in one sheet of ulceration, with a rough and ragged surface and hypertrophy of the other coats, as observed in many cases of phthisis pulmonalis," vol. 1, p. 179. Again, treating of the inflammation of the stomach and bowels he says: "On some occasions we shall see numerous dark-colored distinct points, somewhat elevated with a depression in the centre, which are mucous follicles enlarged; in some places a number of these points will be seen to coalesce, sometimes into a circular space, but in general they are more of an oblong shape. The surface is elevated and sometimes spongy; and upon making a section through this part, it will in general be found that the submucous tissue is principally involved in the disease, and occasionally the muscular tissue also. On looking at the surface through a glass, ulcerations will be discovered. This appearance is most commonly observed in the lower part of

the ileum and caput cecum, in children who have died of bowel complaint. Occasionally numerous distinct points will be observed, as if a penful of red ink had been spattered on the surface of the mucous membrane. * * * On other occasions ulcerations are observed of a circular or oval form, with defined edges, attended by loss of substance not only of the mucous membrane and the submucous tissue, but extending into the muscular coat which may be seen in different places in a ragged state. * * * * Occasionally indeed, the ulceration extends through all the tissues allowing the escape of the contents of the bowels into the abdomen. * * * Ulcerations are sometimes circular sometimes oval; sometimes they run in lines, and on other occasions are found to be irregular in shape. In size they vary from that of a millet seed, to be so extensive as to occupy a large space. Sometimes the whole intestine. * * * Ulcerations of the small intestines are for the most part, found in that portion of the tube most distant from the mesentery," p. 337, *et seq.* Again, speaking of *Tabes' Mesenterica* he says: "At other times the lower part of the ileum and cecum are affected; and occasionally ulcerations of the jejunum, increasing in number however in the course of the ileum," p. 353.

In the forty-fifth volume of the Edinburgh Medical and Surgical Journal are some observations on continued fever as it occurs in the Glasgow hospitals, by Dr. Robert Perry, in which he considers the intestinal lesion as an accidental complication of typhus fever, and not less frequently of small pox; and says that in the latter disease, the morbid appearances in the intestine are the same as those which occur in dothinenteritis itself, which disease, he says, may exist as an affection *per se*, characterised by its peculiar symptoms."—*Bartlett*, 296. It is true that Professor B. questions this testimony; but from what I have seen I am by no means certain of its

want of truth *e. g.*—in 1845 I had a patient who had been complaining for a week with great muscular debility, occasional chills, frequent, weak pulse, tongue whitish with red papillæ, buzzing in the ears; distended tender abdomen, considerable diarrhea, dejections of dirty yellowish color and cadaverous odor, which in fact for two days I considered a pretty fair case of typhoid fever; but luckily, on the third, the eruption of measles undeceived me. If this man had died, I think it quite probable that the lesions of typhoid fever would have been found; if not ulcerations, that equivocal thickening which could very well have been interpreted to be the genuine affection.

Treating of *painter's colic* Mackintosh says: "The following is an abstract of the appearances found on dissection, in the bodies of a number of individuals who died of this affection in the hospital of Beaujon under the care of M. Renauldin: redness and ulceration of the mucous coat of membrane of the alimentary canal, and often an enlargement of the mesenteric gland corresponding to the inflamed and ulcerated portions of the membrane. The redness varied from that of bright rose even to violet or brown. It was disposed in points, in patches, in streaks, and sometimes occupied an extent of several feet. The thickness was variable. The ulcerations were found almost always towards the termination of the small intestines, near the valve of the colon, which was sometimes destroyed; and in cases where diarrhea prevailed, ulcerations were found in the colon; and sometimes they were observed in the stomach. They were deep and numerous, and sometimes the stomach and intestines were perforated." "Roche and Sanson inform us that M. Renauldin had two hundred and seventy-five cases in the years 1821, '22, and '23."—*Mackintosh's Prac.*, v. 1, p. 291.

"In teething children the glands are affected in precisely the same manner as in typhoid fever. Dr. Hale has

seen six cases in his practice, of children who have died during teething in whom the glands were thus affected."—*Am. Journ. Med. Scien.*, v. 25, p. 399. It will be noted, that Dr. Hale is one of those who has contributed most to our knowledge of typhoid fever, and as such, his observations are entitled to much weight.

"Dr. Marizini has written to the Academy of Sciences of France, that he has found the alterations of the intestinal follicles characteristic of typhoid fever, in the body of a seven months child, who died twenty or thirty minutes after birth. Many physicians witnessed the autopsy, and can testify, he adds, to the truth of the statement.—*Amer. Jour. Med. Scien.*, n. s., v. 4, p. 173.

As this disease is to me a subject of great interest, I hope I shall be excused for introducing here, a few notes of cases, which may at first sight seem irrelevant, which however I think not altogether without analogy to our present subject. They are from Abercrombie's Treatise on the Stomach: *Case ninety-first*, from symptoms and appearances on dissection, was pretty clearly one of typhoid fever. *Cases* ninety-third and ninety-fourth were doubtless those of perforation of the bowel occurring in the same disease. *Case* eighty-fifth seems to have been of the same character. At the commencement, after a dose of castor oil, (there being great uneasiness in the abdomen,) he had numerous evacuations consisting mostly of blood. This was on the 8th of October. 9th. Uneasiness of abdomen, chiefly referred to lower portion. Stools still almost entirely of blood. 10. Pulse nearly natural; much pain and tenderness in lower part of abdomen; some dysentery. The evacuations now more abundant in quantity, and remarkably changed in character, being watery, dark colored, and with a remarkable and peculiar fetor." They are compared by Mr. White to the washing of putrid flesh. "After the diarrhea, a regular decline with adynamic symptoms, and died on the 16th." This case

seems to have lasted only ten days. Upon inspection, the whole colon, from its caput to the extremity of the rectum, was greatly and uniformly distended. From the anus to the middle of the arch, the intestine was of a uniform dark color, as if gangrenous. From the middle of the arch to the caput coli, the appearance was more healthy, but was variegated by numerous patches of deep red or livid color. The intestines being laid open, the mucous membrane from anus to middle arch, of deep, uniform dark color, soft, and easily separated; muscular coat in same condition; peritoneal coat healthy. From middle arch to cecum, there were elevated irregular patches of a dark red color, with interspersed portions of more healthy color. Towards the caput coli there was an appearance of erosion or superficial ulceration; and on the inner side of the caput coli were several distinctly defined ulcers. The ileum for a few inches from its junction with the caput coli was slightly distended and its mucous membrane reddened. Other parts of the canal healthy." The author remarks: "It may be of importance to state that the wife of this man was affected with this same disease in a very protracted form, from which she had not entirely recovered at the end of two months. Three sons were also successively attacked, of whom one died." *Case* eighty-sixth fatal on the ninth day after a fair set of symptoms indicating typhoid fever. The mucous membrane was healthy in the stomach and in the upper portion of small intestines. In the ileum there began to appear spots of increased vascularity, which were at first at considerable distances, but afterwards became more numerous; and for about twenty-four inches of the lower end of the ileum, the whole mucous membrane was of a uniform deep red color, without any remarkable change in its structure. In the caput coli the same dark, red state of the membrane continued; and it was here covered by numerous well-defined ulcers, some of them of the

size of a sixpence. In the ascending colon, there was a more irregular state of disease, consisting of wandering undefined ulceration, variegated with dark fungoid elevations of portions of the mucous membrane. In the arch of the colon the disease assumed a different character; for it there consisted of small well defined ulcers, the size of a split pea or smaller; they were quite distinct from each other, and the mucous membrane between them was of a pale color and quite healthy. In the descending colon, the whole of the mucous membrane showed one continued surface of disease—being of a dark-brown color, fungoid and spongy without any defined ulcerations. *Case* eighty-ninth, dysentery, stools copious, varied very much in appearance, being sometimes slimy, sometimes watery, and sometimes consisting of mucus mixed with green matters of various shades. *Autopsy.*—Some superficial ulcerations towards the lower extremity of the colon, but the principal seat of disease appeared in the caput coli, in which were numerous fungous projections ulcerated upon the surface. In the ileum four inches from its lower extremity, there was a portion in a state of recent inflammation, covered with a false membrane. Small abscesses in the liver; mesenteric glands enlarged.

Here we have a lot of cases, some of which, by symptoms and appearances on dissection were pretty certainly cases of what would now be called typhoid fever. Some in which the symptoms would seem to indicate that to have been the disease, but the dissections did not show the peculiar lesions of typhoid fever; but either a corresponding lesion situated in the colon or an analogous one situated in the ileum. The last is considered dysentery by the author, but it seems doubtful if it was not truly one of typhoid fever. At any rate the appearances on dissection were analogous to those having a striking resemblance to typhoid fever.

Whatever others may think of the light in which I have viewed these cases, I am of opinion that a patient and careful examination of them, and of some others of the same author, with an eye to their analogy to typhoid fever, will amply repay the man who makes it.

But it may be urged against many of these cases, that they are not described with sufficient minuteness to enable us to form a proper opinion as to their value. That English writers do not discriminate between typhus and typhoid fevers. That many cases which have been described as infantile remittent fever, and even cholera infantum, were in truth typhoid fever. In one word that things identical have been considered as altogether dissimilar. To prevent, on the one hand confounding things which are really different, and on the other separating those which are alike, is many times a work of great difficulty. Such is the matter now in controversy.

Let us recapitulate this section somewhat. I have declared that the typhoid eruption, the rose spots, though diligently sought for, had not been found in this vicinity. I have exhibited proof, that in some considerable proportion of cases, say from one-tenth to one-fourth, it is admitted to be absent. That in some cases, (whether of typhoid or typhus fever it matters not,) both these and petechiæ appear upon the same patient. That in some cases they are convertible into each other—running from rose spots to petechiæ and *vice versa*. That the lesions of the elliptical plates have not been found where, from the symptoms, we had a right to expect them. That these lesions have nevertheless been found, where we had no right to expect them. That in other cases we have found analogous lesions in the ileum, and corresponding lesions situated in the colon.

It may be argued that the case quoted from Broussais was one of genuine typhoid fever, occurring after one of intermittent fever; or that it was typhoid fever from the

beginning. The history of the case as given does not seem to warrant that conclusion. Still the author may have had his attention so fixed upon some features of the case, as to prevent his seeing others, important to the proper understanding of it.

In the cases from Horner there is evidently no suspicion entertained by the author, that anything of what is usually called fever, affected the patients. Matthew's case, given before, I considered one of pure consumption and there was nothing to change my opinion; and I have the satisfaction of having a confirmation of this opinion in the judgement of Professor Bartlett.

Perhaps it would not be uncharitable to suppose that some of the cases reported as infantile remittent, or even as cholera infantum were in truth those of typhoid fever; as I cannot conceive that it would be difficult to confound them; but I should suppose painter's colic ought not to be so confounded.

Upon a careful examination of the whole ground, shall we conclnde that the lesions of Peyer's plates are invariable and characteristic of typhoid fever—that they are to be found in no other disease? Truly I cannot bring myself to do so. Shall we conclude that they do not indicate a distinct form of disease or even a distinct disease? I am not prepared for that either. Shall we say that they are an accidental symptom which may occur in many different diseases, and even exist independent of other symptoms? It behooves us to be cautious in this matter. In our profession we have had a sufficiency of hasty conclusions, drawn from insufficient observations, which have been replaced by other conclusions drawn from facts equally unsatisfactory. I hope we shall leave this matter under investigation, until we shall have accumulated facts sufficient to enable us to come to conclusions, which shall remain permanent.

(To be continued.)

ART. II.—*On Watering Streets.* By THEODORE S. BELL, M. D., of Louisville, Ky.

I cannot believe that an evil temper, and abusive language are capable of advancing the interests of medical science, nor that they are conducive to a philosophical investigation, and in examining a paper written by Dr. S. Jackson, for the September number of the *Medical Examiner*, and entitled: "Friendly hints to a Reviewer," I shall treat it with as much deference and respect as though it were entitled to be approached with those feelings. No other spirit than that of an inquirer after truth, or of an advocate for it, should ever animate a medical writer, and truth needs none of the extraneous aids of anger, nor abusive language.

The subject of hygiene is one that deserves the best efforts of medical minds. It is a basis upon which medical science may build a superstructure that will bid defiance to all the angry surges that may be sent against it—the foundation is a rock, and cannot be moved. The cultivation of public hygiene, too, seems to mark the boundary line between the empire of true medical science, and the narrow domains of empiricism. We know of no system of quackery, from the days of Perkins's tractoration, down to the most recent phase of empiricism that makes any attempt to guard the people from the causes of disease. The devotees of the various systems of quackery, in the midst of boundless pretension to the cure of all diseases, seem to prefer the prevalence of sickness, and act as though they considered cure better than prevention.

In a proper spirit of investigation I undertook, in the July number of this Journal, to examine a paper, "On Wetting the Streets," read by Dr. S. Jackson, before the Philadelphia County Medical Society. I weighed the statements of that paper carefully, and found them wanting in some essential elements of truth. And Dr. Jackson's article in response has been fully examined, without finding that its author has either

fortified any one of his positions, or advanced the interests of a principle for which he contends. No proof has been furnished which shows that sprinkling the streets of a city, with pure water, has any deleterious effect. The "story" of Capt. Murray's ship, the capital on which extensive operations are attempted, does not prove that the drying operations referred to, had any special agency in preserving the health of the crew of his vessel. Various other agencies are detailed by Capt. Murray, and they are entitled to a more conspicuous place in Dr. Jackson's reasonings, than his *asserted*, but unproved dryness of the atmosphere of the ship. As a proof of this fact, we know that Capt. Ross accomplished the same results that Capt. Murray claims, in the same regions that Capt. M.'s ship occupied, without the use of brodiestoves, or any substitute for them. If the same result is attained without the use of a special agent, that was gained in the company of that agent, we are safe in saying it had nothing to do with the result.

In proof of the fact that a mere humid condition of the atmosphere is not essentially detrimental, the observations of medical men, on the effects of fogs were referred to. Surely a foggy atmosphere is humid enough. But Dr. Jackson takes fire at this, and in his heat strikes off the following: "But why name fogs, *a word that is not to be found in my whole paper.*" A little coolness and calmness might have saved Dr. Jackson from this statement. Let him compare the assertion italicised in the above quotation, with the following extract from page 327 of his original essay: "In every campaign, he says," [Dr. Pringle,] "no epidemic ever ensued on the greatest heats till the perspiration was stopped by wet clothes, wet beds, dews, or *fogs.*" That looks something like finding the word fogs in Dr. Jackson's paper.

There is too much of this kind of assertion in Dr. Jackson's "Hints to a Reviewer." Take another instance. Of the statement made in my former paper—"the humid state of Torquay is notorious," Dr. Jackson says: "Sir James Clarke, the highest authority says otherwise," and Dr. Clarke, quoted

by Dr. Jackson, proves just what I said. Read him—"hence while Torquay possesses all the advantages of the climate of this coast, its chief disadvantage, *humidity*, is felt in a less degree than elsewhere."

Dr. James Johnson, a name that will be held in reverence, as long as medicine is cultivated as a science, thus writes on dry and humid atmospheres: "The very circumstance, in short, which forms the charm, the attraction, the theme of praise in the Italian climate, is that which renders it dangerous, because deceitful—namely, the *long intervals of fine weather between vicissitudes of great magnitude.* This is the bane of Italy, whose brilliant suns, and balmy zephyrs flatter only to betray."

Again: "Italy boasts much of the dryness of her climate. In some places, as at Pisa, there falls as much rain as in Cornwall. In Rome, about one-third less of rain falls than at Penzance, and the number of rainy days is one-third less—being about 117 in the year. This is a poor counterbalance for the *steam* of the sirocco, and the oppressive stillness of the Roman air. *The fogs of England* and its cloudy skies furnish constant themes of querulous complaint; but they would be rich treats in Italy, as defences against the torrents of liquid fire that pour down on her vales from a nearly vertical sun."—*Change of Air*, p. 262.

If it is objected that the sirocco of Italy is a hot moist air, let it be remembered that it is not as terrific in its effects as the hot, dry simoom of the Lybian desert.

In answer to the statement that invalids in the West India islands, were advised by Dr. James Clark to spend their time on the water, Dr. Jackson says: "As to the people seeking their safety on the water, this is merely the less of two evils, for surely no one is crazy enough to pretend that a ship in the torrid zone is a healthy situation." Now Dr. James Clarke, announced by Dr. Jackson, as "the highest authority" advises that invalids may be sent to the West Indies, provided they spend their time on the water. And if any one is unfortunately the victim of lunacy for saying that a ship in

the torrid zone is a healthy situation, what becomes of the "story" of Capt. Murray's ship, which plays such a conspicuous part in Dr. Jackson's two papers? When Capt. Murray's ship appears to sustain Dr. Jackson's hypothesis, the high health of the officers and crew in a torrid zone, is triumphantly appealed to, but when I give Sir James Clake's statement, that invalids may reap advantage by visiting the West India islands, provided they live on the water, Dr. Jackson declares that no body but a crazy man would say that a ship in the torrid zone is "a healthy situation." Alas! the legs of the lame are not equal.

Dr. Jackson quotes Dr. Yandell's very correct remarks about the condition of the streets of Louisville, as an answer to my statement that the streets most constantly wet are the healthiest streets. This is rather straining the matter. There is no contradiction between the statements made by Dr. Yandell, and those I made. He refers to the rapid drying after rains. I wrote of streets that are kept wet by water carts. My facts are fatal to Dr. Jackson's hypothesis, and I repeat them. Large portions of Main and Market streets, and a number of the cross streets that intersect them are kept almost constantly wet, through the summer, and make quite as respectable an approach towards the manufacture of an Italian sirocco, as Philadelphia does. These streets are much more healthy than a large, dry area in the suburbs is at this time, and surely more of Dr. Jackson's "steam" is made on the immense wet surface I have described as the healthy district, than can be made by one or two small marshes, which are to be found in the large dry area. If Dr. Jackson can make his hypothesis tally with these facts, it behooves him to do so. If he cannot he must acknowledge his error, and yield the question. He must meet the facts with truth, and not attempt to cover his retreat, by unwarrantable interpretations, of what he calls "cant phrases" among Western men, and by perverting language to suit his own views. When I said "a considerable part of the city retains its health." I meant just what a correct Philadelphia medical writer would

mean by the same language—I said, and meant that the considerable portion of the city referred to, enjoyed good health—there was not sickness of various kinds in every part of the city, as Dr. Jackson's imagination induces him to represent the fact.

I have thus answered the leading points of Dr. Jackson's second paper on wetting streets. There are various things in that paper, which have no kind of bearing on the hygienic question before us, and I shall let them pass to that inevitable bourne for which they are fitted. There are wild and reckless assertions too, such as his denial of his admission that streets may be safely sprinkled with pure water, upon which it is unnecessary to waste time. But I regret that in Dr. Jackson's anxiety to be fierce, he fires a broadside at the city of Louisville, and endeavors to show that the people are graceless outlaws. He says:

"Have not the people of Louisville built a huge bridge which the law will soon compel them to unbuild? This however is not so much a murmuring as an open rebellion, for the New Testament enjoins all men to obey the laws." Now Dr. Jackson is respectfully informed that Louisville has not built the huge bridge of which he speaks. That bridge is at Wheeling, Virginia, which is several miles from Louisville. But what this had to do with the hygienic question, I am at a loss to know. It is, however, in perfect keeping with that medley of Greek, Latin, French, and—I am at a loss to name the other language, that flourishes over nearly sixteen pages of the Medical Examiner, under the name of Dr. S. Jackson.

ART. III.—*Jarvis's Adjuster for the Reduction of Dislocations, and Treatment of Fractures.* By THEODORE S. BELL, M. D., of Louisville, Ky.

In a brief editorial notice, in the last number of this Journal, reference was made to an admirable apparatus, entitled Jarvis's Adjuster, and its merits are such, in my estimation,

as to entitle it to the confidence of the medical profession. In order to present its claims fairly, and to advance the usefulness and certainty of surgery, I undertake a description of the instrument, and of its powers. As a preliminary to this, however, it will not be amiss to notice the subject of dislocations and fractures.

In the preliminary remarks of Sir Astley Cooper, to his invaluable work on Dislocations and Fractures, that distinguished writer endeavors to impress upon the minds of surgeons the exceeding importance of a correct understanding of the pathology of dislocations. There are many deplorable examples of mistakes, blunders, and failures, in almost all sections of the world, growing out of this very ignorance, and an unreduced dislocation is a monument, to a surgeon, as durable, as far as his surgical life is concerned, as brass or marble. No man should attempt the reduction of a dislocation, unless he has a thorough and minute understanding of everything that he may have to encounter in the reduction. If an extending force is improperly applied, an immense amount of unnecessary power may be exerted, where little is required, if it is correctly, judiciously, and skilfully directed. Columbot undertook, some years since, to show that no mechanical aid, and no assistants of any kind were of use in the reduction of any kind of dislocations. He made the patient exert the counter-extending power, while the surgeon skilfully rotated the head of the bone to its proper place. Many respectable surgeons then imagined that the ambe of the ancients, and the pullies, and the stout assistants of the moderns were about to be consigned to oblivion. But a little experience caused the illusion to vanish—the pullies have maintained their place, and deserve some reputation.

The importance of a thorough understanding of the pathology of the shoulder joint dislocation is so great, that no apology is necessary for introducing here the excellent remarks of Mr. Crampton on the subject, which were published in the seventh and eighth numbers of the Dublin Journal. Sir Astley Cooper gives, in his work on Dislocations, but two

dissections of luxated shoulders, and both of those were dislocations into the axilla. Mr. Crampton's investigations shed a good deal of light, however, on other kinds of dislocation of the shoulder, and the pathological appearances in the same species of dislocation, as that described by Sir Astley, vary somewhat from Sir Astley's description. It is well enough for the surgeon to remember a portion of Sir Astley's description. He found that "the resistance to reduction (even after death,) was such as he could not by himself overcome; he divided one muscle after another, cutting through the coraco-brachialis, teres major and minor, supra-spinatus muscles, but still the opposition to his efforts remained; he next divided the deltoid muscle, and found that the supra-spinatus muscle was his great opponent, *until he drew the arm directly* upwards, when the head of the bone glided into the glenoid cavity."

A surgeon who has had much to do with dislocations of the shoulder, cannot fail to place the proper estimate upon such facts as these, and by way of impressing the subject upon the attention of the reader, the investigations of Mr. Crampton are added.

Mr. Crampton dissected a dislocation of the shoulder, caused by an injury which produced the death of the sufferer in two hours after he met with the accident. "When the axilla was cleared, the head of the os humeri was seen lodged on the inferior costa of the scapula, or rather on its neck; the head of the bone, in escaping from its socket, had pushed the teres minor downwards, and burst through the lower part of the subscapularis muscle, some of the fibres of which closely embraced the neck of the bone, while the bulk of the muscle was pushed upwards and detached from the inner surface of the scapula. The neck of the humerus, therefore, was in some degree embraced by the divided fibres of the subscapularis muscle, while a portion of its head rested on the neck, and part of the venter of the scapula, without the intervention of any muscular substance. The short head of the biceps and the coraco-brachialis were forced to describe a curve outwards over the neck of the humerus on the sternal

side, while the long head of the triceps crossed the neck of the bone obliquely on the dorsal side; this strangulation of the head of the bone, by the surrounding muscles, was made most apparent when extension was applied to the fore-arm. The biceps and triceps seemed then to close behind the head of the bone, and interpose themselves between it and the glenoid cavity; the tendon of the long head of the biceps remained in its groove, but the sheath in which it runs was partially ripped up.

"The capsular ligament was completely torn from the lower part of the neck of the humerous to the extent of more than half its circumference, the torn edge appearing like a crest over the head of the bone. * * * The tendons of the supra-spinatus, infra-spinatus, and teres minor were completely torn from the humerus, carrying with them a scale of bone from the tubercle.

"To ascertain the obstacles opposed to reduction, the scapula was fixed, the arm raised, and extension applied to the wrist. So long as the hand was held *supine*, the head of the bone remained immoveable, apparently from the closing of the biceps and triceps behind it. On turning the hand *prone*, rotating the limb inwards, and continuing the extension, reduction was easily effected.

"The appearances in this case differed from those observed by Sir Astley Cooper, and agreed with those related by Mr. Thompson. In the case of the latter gentleman, the head of the bone was lodged on the inside of the neck of the scapula, between the subscapularis and teres major—the capsular ligament was completely torn from the whole circumference of the humerus—the attachments of the tendons of the supra-spinatus and infra-spinatus were torn off, with the part of the bone they were inserted into—and some fibres of the subscapularis encircled the neck of the bone. In Sir Astley Cooper's case, on the contrary, the tendon of the subscapularis was torn through, but the supra-spinatus and infra-spinatus remained attached to the tubercle, and reduction could only be effected by relaxing these muscles. Mr. Crampton observes very justly that the comparison of these facts is

sufficient to shew, that in apparently similar dislocations of the humerus there may be very different degrees of lesion, and consequently different causes of resistance to reduction.

"J. W. æt. 30, was precipitated twice consecutively into a burning lime-kiln, from a height of about fifteen feet. He was carried to the Meath Hospital. In addition to severe burns and lacerations, there was a dislocation of the humerus under the pectoral muscle, which Mr. M'Namara himself reduced, by merely drawing the arm gently forwards and downwards with one hand, while he pushed the head of the bone towards the glenoid cavity with the other. The man died in the course of the day, and eighteen hours after death the joint was dissected.

"The dislocation was unattended with the rupture of any muscle, or the separation of any tendon from its insertion into the bone: by a slight effort the dislocation was reproduced, and the pectoral muscles being removed, the polished head of the bone was now seen lodged on the cervix of the scapula, at the root of the coracoid process, but extending nearly as far as the notch in the superior costa; it had passed out through a rent in the capsular ligament, *over* the *upper edge* of the tendon of the subscapularis, detaching this muscle from its connexion (which at this point is but slight) with the inner face of the scapula, and pushing its fibres downwards, so that they formed a curve which partly embraced the neck of the humerus; the supra and infra-spinatus muscles were on the stretch, but had suffered no injury. The cellular substance covering their tendons was deeply ecchymosed, so as to mark their course most distinctly. On replacing the head of the bone, the opening in the capsular ligament, through which it had escaped from its socket, could be distinctly seen. It was formed by a separation of the ligament from the interior side of the brim of the glenoid cavity from top to bottom; it was bounded at the top by the tendon of the supra-spinatus, and at the bottom by the inferior edge of the tendon of the subscapularis, the rent was continued as far as the root of the lesser tubercle of the os humeri, and was of sufficient extent, but no more, to permit the head of the bone to

pass easily through it; the *inferior part of the capsular ligament, however, (the part corresponding with the axilla,) was perfect.*

"The great blood vessels and nerves lay to the sternal side of the head of the humerus, and were forced a little out of their course. The axis of the head of the bone, in its dislocated position, was scarcely a quarter of an inch higher than the axis of the glenoid cavity.

"Mr. Crampton appends to these interesting and valuable facts some practical observations. We will endeavor to give their substance.

"I. Resistance to the reduction of a recently dislocated shoulder would seem to be owing to spasmodic contraction of the muscles, and *not* 'from the neck of the bone being tightly embraced by the ruptured capsular ligament.' Dissection does not prove the latter, and therefore it is mere supposition. Muscular contraction then being the obstacle, we must diminish its power by bleeding, the warm bath, antimony, long-continued extension. Another excellent means of obviating muscular oposition is taking the muscles by *surprise.*

"If before assistants are called in, or any apparatus is applied, the surgeon, while he appears to be occupied merely in ascertaining the nature of the injury, applies a gentle extension at the wrist, and slowly raising the arm to nearly a horizontal position, suddenly pulls it upwards and a little forwards, (that is towards the patient's face,) while at the same time he as suddenly pushes the trunk backwards by pressing, with the left hand, below the axilla, he will in a great number of *recent cases* succeed by this simple process in reducing the dislocation. His success, however, will in a great measure depend on the *unexpectedness* of the attempt; he should, therefore, endeavor to divert the patient's attention from his proceedings, and I know of no means so effectual for this purpose, as inducing him to describe circumstantially everything connected with the occurrence of the accident; this is a theme on which all patients, who are at all able to express themselves, are sure to expatiate with the greatest satisfaction, and

once engaged on so engrossing a topic, it will require but a small degree of tact on the part of the surgeon to seize the favorable moment when he can apply his force with the greatest advantage.'

"II. In luxation into the axilla, muscular contraction seems to operate chiefly by pressing the head of the humerus against the inferior part of the brim of the glenoid cavity; in Sir Astley Cooper's case this was mainly effected by the supra-spinatus. The obvious deduction is, to raise the arm to nearly a right angle with the body previous to extension, and not to use any force in pressing the head of the humerus upwards, as that must lock it more firmly.

"III. The *direction* of the extension is now generally that of a right angle with the body. In the reduction by the heel in the axilla the direction is nearly parallel with the body, but Mr. C. thinks that more force is necessary with this method. Mr. White, of Manchester, drew the limb directly upwards, but the practice has fallen into disuse—a presumptive proof of its not possessing any superior efficacy. The method has lately been brought forward at the Hôtel Dieu as a novelty. We are not surprised at this. M. Dupuytren has no love for English surgery, and candor forms no part of his merits or acquirements.

"IV. Extension is now pretty generally applied to the wrist in preference to the arm. It is less painful, if not more effectual.

" 'V. Great stress is laid by most surgeons on the advantage of fixing the scapula, as it is called; it may be doubted, however, whether the thing be possible, or if possible, advantageous. It is quite plain that a split cloth, or a napkin with a hole through which the arm is passed, can, when the arm is strongly extended, acts only on the inferior costa of the scapula, or rather on the walls of the axilla formed by the edges of the latissimus dorsi,

teres major, and pectoralis major muscles; the whole effect of this force can be no other than to push the inferior angle of the scapula backwards and upwards, consequently to direct its superior angle, and the glenoid cavity downwards, and, by acting on the pectoralis major and latissimus dorsi, to draw the head of the humerus inwards towards the ribs, that is, to remove it *from* the glenoid cavity. To obviate this objection, some surgeons recommend pressure to be made by the hand of an assistant on the acromion of the scapula, so as to push it backwards while the humerus is drawn downwards and outwards; but it is plain that unless the force which the surgeon applies to the head of the scapula to keep it back, be at least equal to the extending force which is applied to the arm, the scapula cannot be *fixed*, it *must* follow the arm; besides when the arm is raised, the deltoid fills up the sub-acromial space and renders it impossible to apply any appreciable force to the acromion. As the neck of the scapula cannot be pushed *upwards*, it is proposed by Bonn, to disengage the bones by pressing the head of the humerus *downwards*, at the moment when the extension is at the utmost; the proposal is a most rational one, and has been adopted for several years past in the County of Dublin Infirmary, as I think, with considerable advantage.'*

"VI. When a considerable power of extension is required, Mr. Crampton prefers a ladder to the pullies. One end of the ladder is fixed—the patient stands between the bars near this end—the wrist of the affected limb is tied by a jack-towel or handkerchief to the sides of the free end of the ladder at a convenient distance from the body—and thus a powerful lever is obtained,

*Contrast this with Mr. Toogood's brief remarks on fixing the scapula.

one end of the ladder fixed on the ground being the fulcrum, the other end being free, and its depression, by an assistant producing the extension. Counter-extension is, of course, effected by a towel under or in the axilla.

"VII. It has been much mooted whether dislocation forwards is a *primitive* dislocation, and it has been supposed that every luxation is primarily into the axilla. The case related by Mr. Crampton seems to prove that it is, or at least in that instance it was a primitive dislocation, and as the inferior part of the capsule was not ruptured, it would of course have been injurious to have first reduced the head into the axilla. In such a case the clear indication is to force the head of the bone *backwards* towards the glenoid cavity, the axis of which is as nearly as possible in a line with that of the head of the humerus; this can be effectually done by applying a fulcrum immediately below the axilla, and using the dislocated arm as a lever of the first kind; the surgeon should therefore place his left arm extended horizontally, immediately below the walls of the axilla, between the dislocated arm and the chest, and then grasping the wrist in his right hand, he should draw the arm forcibly across the patient's body."

These remarks are copied from the Medico-Chirurgical Review for 1833, and the practical bearings of the pathological facts must fasten themselves upon the mind of the surgeon. There are many dislocations of the shoulder that are manageable almost without any mechanical contrivances, but there are others that resist all the ordinary methods, and it is in cases of this kind that Mr. Crampton's pathological facts rise to importance. Malgaigne has described a luxation of the humerus, which has been considered an impossible form for that accident. The head of the humerus was carried forward and upwards, above the coraco-acromial ligament and coracoid process. The projection was so great that it extended

for two inches beyond the anterior surface of the clavicle; and it was so superficial that, at some parts, it seemed subcutaneous." Malgaigne's opinion was, "that the head of the bone had at this spot separated the deltoid and great pectoral, so as to become almost subcutaneous." This is certainly a very remarkable case of dislocation, and shows the necessity of fullness of knowledge upon all possible varieties of dislocation of the shoulder, and upon all the obstructions that interfere with reduction.

Professor Rozer, of Tubingen, describes a variety, discovered in a dissection made by himself, in which the head of the humerus laid in front of the short head of the biceps, the sub-scapular muscle was ruptured, and completely detached from the lesser tuberosity of the humerus, and the head of the bone elevated the scapular extremity of the pectoralis minor. This dislocation had taken place seven years previous to the death of the subject, and remained unreduced. The obstruction to the reduction was the interposition of the short head of the biceps between the head of the humerus and the glenoid cavity. It could have been reduced by proper rotation.

I do not purpose to enter into a disquisition at this time, upon the many means in the way of mechanical contrivances, that have been proposed for reducing dislocations of various kinds. The great desideratum in these matters is that which will most effectually overcome the resistance of the muscles up to a certain point, which gives the surgeon the utmost freedom of motion in the luxated limb, and which enables him, at the proper moment, to cease the extension of the muscles, and instantly to command their services in the reduction of the luxation. Mr. Spong, Mr. Skey, and Dr. Gilbert have proposed various measures for these objects, but their methods looked to extension power, and neglected by far the most important object. They antagonised the muscles admirably, but could not then seize upon their aid at

the moment it should have been given, nor could they enjoy the proper amount of freedom in moving the limb, for the purpose of clearing away obstructions. The pullies are open to the same objections. Mr. Epps, of University College, London, in 1845, made a partial approach to the invention of Dr. Jarvis, but his apparatus was defective in so many particulars, that it made no impression upon the surgeons of the day.

The profession has, in the pullies, an apparatus of sufficient power for extension, and counter-extension. If those objects were all that are involved in the reduction of dislocations, the pullies would leave nothing to be desired. But in every form of luxation there are more important objects than extension and counter extension; in some dislocations of the hip, the extending qualities of the pullies are to a considerable extent, a disadvantage, because of their interference with other necessary measures. Mr. Vincent, an accomplished surgeon, seems to have laid down the principle of action, that should guide in nearly all dislocations. He says: "The surgeon cannot be said to effect the reduction. His efforts are limited to drawing the bone so near the glenoid cavity as to bring the muscles into a situation to remove that association in which all these actions have been accustomed to take place; in other words, in which they all concur to draw the bone into its normal position and retain it there." The true principle is to command the contractile power of the muscles, so far as it is exercised upon the abnormal position of the bone, and at the moment the bone is ready to resume its natural place, the aid that overcame the resistance of the muscles should be cut loose, to use a figurative expression, and the muscles will then perform their duty.

The following remarks, copied from the London Lancet, January 3, 1846, present the subject in a very forcible point of view. The Lancet says: "Dr. Jarvis re-

marks that the first object in the reduction of dislocations is to overcome the resistance offered by the muscles, by their power of approximating their points of attachment, for which extension and counter-extension are necessarily employed, and either manual assistance or the pulleys are resorted to, both of which act mechanically alike in producing traction on the limb; attention is then drawn to the fact that they both act from fixed points over which the surgeon can have no control, so that if the line of extension happen to be correct he does not desire it changed, but if it happen to be incorrect the force must first be relaxed before he can change it.

"Thus in luxation of the femur into the ischiatic notch, in consequence of the oblique position which the head and neck occupy to the shaft of the bone, any force applied to the head from the other end of the shaft and in a line with it, necessarily forces the head in some degree (while passing through the muscle) to follow the obliquity of the neck, and not the line of the shaft, thus making an oblique track from the cotyloid cavity. Suppose pulleys applied, the head and neck being forced obliquely into the muscular tissue, as not unfrequently happens, we have power constantly and steadily operating from fixed points, and before the direction of that force can be changed we must relax it. The head and neck, from their oblique position to the shaft, especially since the head is larger than the neck, would be likely to operate like a hook around a portion of muscle which lies directly below them, and before they could be disengaged, the force operating on one line would be likely to tear through the muscle or break the bone—a case which has happened more than once. But if, instead of the extending point being fixed, as in the case of the pulleys, it is left free, and subject to the command of the surgeon, while the force is steadily and constantly applied, the surgeon would be much more likely to disengage the head by giv-

ing free motion to the limb, than he would by applying that force in one given line. Here is another error in applying mechanical principles to surgery.

"In some dislocations of the os humeri, where the head of the bone is driven into the soft parts, and the neck is closely embraced by them, we must first disengage the head before we can hope to succeed in reduction. With the pulleys, force is steadily applied, but owing to the firmness with which the neck is embraced, and the strength with which the muscles contract, the bone is not relieved from its confinement. If the extending point was subject to the will of the surgeon, to move at pleasure, while the force was yet constant and steady, the giving free motion to the limb would greatly aid him in reduction, and that too without increasing the power.

"In the four dislocations of the femur, Dr. Jarvis' instrument may be applied so as to have any amount of power available which we require, and in whatever direction we need, and at the same time to leave the limb just as free for motion as it was without the instrument attached to it. All the indications required are fulfilled without the extending point being fixed, as by the pulleys or with assistants. In a dislocation of the bone upwards and outwards, the head resting on the dorsum ilii, if you wish to move the limb to disengage the head of the bone from the gluteus minimus, into which it has been driven, while all the power which you have applied is retained, you can do so. The limb can be moved with the same facility as before the instrument was applied. The head of the bone can be thrown from the dorsum ilii while an assistant rotates the thigh on its own axis, a side lever being at the command of the operator which does not in the least affect the line of extension.

"In a dislocation into the ischiatic notch the side lever is used merely to lift the head of the bone from the sacro-sciatic notch, while the extending force is applied to bring

it forwards from that deep situation to the acetabulum.

"In a dislocation on the pubes the head of the bone is out of the line of the upper third of the ilium and the inside of the knee, some two or three inches; it lies above a transverse line passing through both acetabula about an inch. The greatest amount of force to reduce this dislocation is generally required to operate transversely to the body, yet much force also is required to restore the normal length of the limb. With this instrument properly adjusted, while the extending force is applied, the instrument and limb may be gently but firmly carried outwards. The instrument itself becomes a lever, a strap crossing the dorsum of the ilium the fulcrum and the head of the bone the body to be moved, and the surgeon gently applies his own force to the end of the lever as the moving power.

"By an entire change in the application of the power the instrument is made available in luxations into the foramen ovale."

"In the 'Gazette des Hôpitaux,' Dr. Huet states that "in Dr. Jarvis's instrument we have a means of extension and counter-extension, powerful and at the same time regular, subject to the will of the operator, and easy of application. There will no longer be occasion to look around for fixed points of extension and counter-extension, these points departing directly from the centre of motion of the instrument. The operator will have at his disposal an enormous power which the most vigorous muscles cannot resist. He may increase it, diminish it, apply it rapidly or slowly, according to the resistance of the muscles, without danger of injuring them in any manner; and he can act with facility upon the mobility of the dislocated limb during the whole period of reduction. These and many other advantages are enumerated; we have examined the instrument, the cost of which is ten guineas; we can testify to its ingenuity, and it appears to

us to be correct in principle, and capable of fulfilling the purposes professed; but practical experience alone can determine definitively as to its real utility."

In the reduction of old luxations, these principles are, if possible, more valuable than in recent cases. In every possible point of view, the adjuster of Dr. Jarvis is infinitely the best apparatus I have seen for reducing every species of dislocation that can be aided by any mechanical contrivance, whether it is a luxation of the hip, shoulder, thumb, or fore-arm. The adjuster is available at any moment its services may be needed. As Dr. Huet says, it is unnecessary to hunt up fixed points, as we have to do for pullies. It can be used in an open field, at the road side, or at any other place where accident may require its use. While it gives the surgeon an apparatus for extension and counter-extension, superior to all others, it enables him to move the dislocated limb with great freedom, and to use the muscles, by an instantaneous stoppage of the extension power, for the completion of the reduction. These are the eminent advantages of Jarvis's adjuster, and well entitle it to the confidence of surgeons. If it is properly used, it cannot fail to grow upon the esteem of those who desire its services.

Before dismissing the subject of dislocations, it may not be improper to refer to the experience of the profession on the age of dislocations that may be reduced. Surgical authorities are somewhat at variance with surgical experience on this point. The usual admonition is not to attempt a reduction if the bone has been out of its place more than a month; and this too against the matured experience of Desault! Dieffenbach reported a case in 1840, in which he reduced a dislocation of two years' standing, of the shoulder. Instead of resorting to force, to break up adhesions, he introduced a small knife through the skin, while the arm was well extended, and divided the most tense portion of the pectoralis major;

near its tendon. The knife was then introduced at the posterior part of the border of the axilla, and the latissimus dorsi, and teres major and minor were divided. After this the knife was introduced at three different points near the humerus, for the division of the dense, hard, false ligaments which surrounded the artificial joint. By judicious rotation, the head of the bone was then restored to the glenoid cavity. Proper bandaging restored the limb, in a short space of time, to its usefulness.

Dieffenbach also reports a reduction of a dislocation, of a year's age, of the ancle joint. The reduction was effected by the section of the tendo-Achillies. The cure is reported to have been perfect.

M. Sedillot, surgeon at the Val de Grace, reports a case of luxation of the humerus backwards into the fossa infra-spinata, which was reduced one year and fifteen days after the dislocation occurred.

Dr. Stark, in the Edinburgh Medical and Surgical Journal, for January, 1848, reports a case of dislocation of the head of the radius, successfully reduced two years and one month after the occurrence of the luxation.

It is unnecessary to burthen this paper with many cases of the kind referred to. There is abundant reason even in these, though they stood alone, to encourage judicious, patient and persevering attempts at reducing old dislocations. Surgery should be a progressive science. I am satisfied that with Jarvis's adjuster, and a prudent, carefully guarded administration of pure chloroform, much more success in the reduction of dislocations, both recent and old, may be anticipated, than can be hoped for without those agents. Dr. Gregory has taught a sure method both of purifying chloroform, and of testing its purity, and statistics seem to establish the fact, that if pure chloroform is administered to a person in a horizontal position, and on an empty stomach, there is no danger to be apprehended from its use.

In addition to the eminent advantages of Jarvis's adjuster, in the managent of dislocations, it has many claims to confidence in the treatment of fractures, among those who are wedded to splints, and will not forsake their idols. The adjuster certainly fulfils all the indications, in the treatment of fractures, which the experience of surgeons has deemed essential, better than any splint that has ever been devised. In this Journal, for November, 1849, I quoted M. Bauden's rules of treatment, and demonstrated, I think, that all the apparatus in ordinary use failed to fulfil the objects of the surgeon. These very failures are the basis of most of the suits at law, for mal-practice, and a distinguished Professor has recently claimed Professor Hamilton's statistics of fracture as evidence that juries should not look too confidently to surgery, for success in the treatment of fractures! It is hoped that surgeons will rather look upon those statistics as a most excellent reason for examining into the causes of failure, and for the discovery of better means of treatment. To those who have faith in the use of splints, I would commend the adjuster, as the most reliable and certain aid they can find in mechanical apparatus in the management of fractures. This instrument maintains the most perfect extension and counter-extension, and secures the coaptation of the fractured ends of the bone. Unlike other instrumentation, which does generally quite as much evil as good, the adjuster performs its part without that mixture of evil, which prevails under all the modifications of Desault's splint, and of the inclined plane, as it is generally used.

There are those who object to the adjuster because the inventor protected his rights to his instrument, by the patent laws. I can see but a dim shadow of analogy between cases of this kind, and patent medicines. The latter are the production of an impudent cupidity, that is altogether reckless of the evil that may be done, provided

the pockets of the charlatan are filled. His catholicon is not placed in the hands of those who know how to use it for the benefit of sufferers, but is sown broadcast over the land, to be swallowed by the ignorant and the credulous, for every variety of imaginary or real ailment. The patent right in Jarvis's adjuster has no resemblance to such things as these. It is in the nature of the copy right in a book, or an anatomical atlas, and no one imagines that a medical author does any wrong by seeking the protection of the laws of copy right. If one is professional the other is equally so. The inventor of a useful, ingenious, complicated set of instruments should guard the integrity of his instruments, by securing their manufacture in the best possible style, and he has as good reason for protecting them from piratical instrument makers, as the author has for guarding himself from piratical publishers. When a surgeon uses a patented instrument he knows what he is doing, but when a physician prescribes a secret nostrum he works in darkness, and exhibits a culpable recklessness.

I have merely given a view of the merits of Jarvis's adjuster, and mentioned the objects that can be attained by it. It would be impossible to convey a correct idea of its peculiar construction, without illustrative drawings, and I cannot describe it, beyond saying that the instruments acts upon the pinion and ratchet principle; this produces counter extension, through a fork for the hip, shoulder, or elbow, as the case may be, and the power of extension is perfect. Pads, straps, belts, &c., form a part of the instrument. An inclined plane for fractures is also a portion of the apparatus.

But though I cannot describe Jarvis's real contribution to the progress of surgery, those who may desire to understand the inventor's method of reducing dislocations, and of treating fractures may do so by addressing Dr. George O. Jarvis, Portland, Connecticut, or George

Kellogg, A. M., Birmingham, Derby, Connecticut. The lectures of Dr. Jarvis, published in the London Lancet, in 1846, are among the most sensible teachings in surgical literature, and will amply repay any surgeon or physician who may study them. While the author of those lectures was in Europe he won the favor and approbation of the enlightened professional men of France and England. Sir Benj. Brodie, Mr. Quain, editor of a valuable anatomical work, S. Cooper, author of "First Lines of Surgery," and "Dictionary of Practical Surgery," R. Keate, Surgeon to St. George's Hospital, W. Ferguson, author of "Practical Surgery," Thos. Hodgkin, lecturer on morbid anatomy of the serous and mucous membrane, E. Stanley, President of the Royal College of Surgeons, "The London Medical Gazette," "Gazette des Hopitaux," "Archives Generales de Medecine," and a number of prominent American surgeons, have borne strong testimony to the great merits of Dr. Jarvis's adjuster. I am satisfied that a medical man who makes himself familiar with Dr. Jarvis's lectures, and adjuster, will not willingly do without the instrument. He may secure success in cases, where there is frequent failure, and constant uncertainty without it.

REVIEWS.

ART. IV.—*A Systematic Treatise, Historical, Etiological, and Practical, on the Principal Diseases of the Interior Valley of North America, as they appear in the Caucasian, African, Indian, and Esquimaux varieties of its Population.* By DANIEL DRAKE, M. D. Cincinnati: Winthrop B. Smith & Co. Philadelphia: Grigg, Elliott & Co. New York: Mason & Law. 1850.

(CONTINUED.)

From the Balize, Dr. Drake ascends to the city of New Orleans, and gives a minute topographical description of this great Southern mart, in which he notices its position and plan, its lakes and swamps, its bayous, dikes, canals, street currents and inundations, its forests, the fevers of its swamp, its batture, its boats and shipping, the condition of the city, the composition of its society, its filth and fevers. The position of the city is a remarkable one. It is built upon land but recently reclaimed from the sea, and stands several feet below the river in its highest stages. Its latitude, the humidity of its climate, the nature of its soil, and its proximity to extended swamps, all point it out as the favorite seat of malarious diseases; and yet it is notorious that there are periods in which New Orleans is almost wholly exempt from such visitations. During the winter and spring its salubrity is probably much greater than that of more northern cities; but what is a more curious anomaly is, that its healthiest seasons are those which succeed inundations of the river. Dr. Drake remarks, that while the police of the city has

been improved in latter years, the population has also greatly increased, so that along the river there has been a vast augmentation of foul and corrupting offals. It is along the latter, he adds, that yellow fever is most prevalent, as it is on the side of the city adjacent to the swamps that autumnal intermittents and remittents chiefly prevail. This is not the place to institute the inquiry whether the prevalence of the former in the neighborhood of the river is due to the local causes there abounding, or is attributable to its importation by ships arriving from more southern ports. This question is one which will come up and be discussed under the head of yellow fever.

From the following account of the angle of the Delta of the Mississippi one would hardly expect to learn that it was so exempt as it appears to be from miasmatic fevers:

"ANGLE OF THE DELTA.—This extends from the Bayou Plaquemine to the mouth of Red River, a distance of more than one hundred miles, and includes the important parishes of West Baton Rouge, Point Coupée, and the eastern part of Avoyelles, all in the State of Louisiana. The Mississippi, through nearly the whole length of this angle, flows close to its left bank, which is a continued tertiary bluff. The right bank is raised by a levee, which, however, does not afford full protection from overflows; for the bends in the river are here of the most remarkable kind; and consequently, from the retardation of the current, the levees are apt to give way; moreover, the banks of the Bayou Atchafalaya, which flows through the western part of the angle, are so low, that in the annual rise of the Mississippi they are deeply overflowed. Thus the settlements in this bottom are chiefly on the river bank, with a levee in front, and swamps, which generally dry up in autumn, and bayous, ponds, and lakes, in the rear. This portion of the Delta was settled by the French at an early period; and all the arable land has long been subjected to the action of the plow, and other agricultural implements, with full exposure to the rains

and sun. Cotton, the former staple, has largely given place to sugar. The population of this region is entirely rural. There is not, I believe, a single town; but at the distance of fifty-five miles above Plaquemine, there is a public steamboat landing called Waterloo.

"From all that I have been able to learn, autumnal fever in this portion of the Delta is generally mild and not remarkably prevalent. Doctor Thomas Beaumont, who resided, when I saw him, on the tertiary plateau, several miles from the Delta, in the parish of East Baton Rouge, assured me, that malignant cases of autumnal fever were decidedly more frequent and fatal where he then lived, than in the Delta from which he had removed; and Doctor McKelvey, of St. Francisville, informed me on the authority of Doctor G. W. Smith, who had practiced his profession on the Point Coupée coast, and also on the opposite bluffs in West Feliciana, that the fevers of autumn were milder in the former than in the latter locality."

The medical topography of the great valley affords a number of facts which do not seem quite reconcilable with the commonly received theory of malaria. We take, as an instance, the Yazoo bottom, which is subject to wide annual inundations in the month of May, June, or July. Dr. Drake ascended the Yazoo river in the latter month, and found that the inundation extended, with but little dry land, to the Mississippi, distant thirty or forty miles to the west. "When the flood of the Mississippi subsides, the vast, temporary, and shallow lake is drained through the Yazoo; and before the month of September, much of the surface becomes so dry as to shrink until fissures are produced in the new deposits." All the circumstances most favorable to the production of malignant fevers are here present, and yet the testimony of the physicians of that region is, that the bottom is not more infected than the uplands. Yazoo city is built on a bluff above this bottom, in latitude 32° 40′, having drowned lands immediately opposite to it, in a western direction, across the river. It is thus exposed to the

western, south-western, and north-western winds, all of which traverse this bottom. "Autumnal fever," says Dr. Drake, "prevails at Yazoo, but not beyond the degree of its prevalence on the uplands in the rear of the town." He adds that "although genuine and fatal cases of yellow fever have been introduced from the towns below, the disease has never spread."

No one will question that marshes, river-bottoms, stagnant water, and decaying vegetable matter, have an important agency in fostering disease; but facts such as these, which it would be easy to multiply to almost any extent, go to prove, we think, that we have still much to determine in regard to the laws of malaria. The "American Bottom," famous for its deep, rich soil, lies some degrees north of the Yazoo, but still within the zone of malignant fevers. It abounds in organic matters, and in sloughs, ponds, lakes, and bayous; and yet Dr. Drake did not learn that the French inhabitants, who settled in the bottom many years ago, "had been much affected with autumnal fever." The Americans who have more recently gone there "are sickly in autumn and summer." Dr. Farrer, of St. Louis, was able, in former years, to "distinguish, by their sallow complexion and languid aspect, the people of the American Bottom from those of the country back of the city." The French settlers, too, according to Peck, the historian of Illinois, many of them, "are rather dwarfish and stunted;" in all of which we see indubitable evidences of a subtile poison engendered by a peculiar soil, but which operates diversely in different localities. On the Yazoo bottom it excites a violent autumnal fever, and on the American bottom a mild intermittent, under the slow operation of which the body shrivels, and the face acquires a hue and expression which physicians recognise across the street.

Dr. Drake follows the Mississippi river to the highest settlement, and then recapitulates, in the following table,

the relative prevalence of autumnal fever at the military posts which stand upon its banks:

Posts.	N. Lat.	Distance from the Balize.	Elevation above the Gulf of Mexico.	Ratio per cent per annum of Intermittents.	Ratio per cent per annum of Remittents.
Fort Jackson,	29° 29′	30 m.	4 ft.	114	15
Baton Rouge,	30° 36′	145	90	51	30
Jefferson Barracks,	38° 28′	1,378	460	34	16
Fort Armstrong,	41° 32′	1,722	550	17	10
Fort Crawford,	43° 03′	1,932	720	28	4
Fort Snelling,	44° 52′	2,192	850	4	2

From this table it will be seen, as already remarked, that there is a steady decrease of fever as we proceed north. Fort Crawford is unfavorably situated near the mouth of the Wisconsin, and affords an exception to the rule. Advancing north through fifteen degrees of latitude, and eight hundred and fifty feet of elevation, there is, with this exception, a regular decrease of intermittents from one hundred and fourteen per cent. down to four; and from the second post, Baton Rouge, a regular decrement of remittents from thirty per cent. to two.

This part of Dr. Drake's work affords many instructive illustrations of the bearing of mineral geology upon disease. Passing from the older rocks to the cretaceous formation in Alabama and Tennessee, it is found that fevers are both more common and more malignant. The towns which are included in this formation are Columbus, Georgia, on the left bank of the Chattahooche river; Montgomery, Wetumpka, Selma, Cahawba, Marion, Greensboro, Demopolis, Eutaw, Tuscaloosa, and Pickensville, in Alabama; Memphis, Somerville, Randolph, and Bolivar, in Tennessee; and Columbus, Yazoo, Pontotoc, and Holly Springs, in Mississippi. The whole of this region, according to Dr. Drake, who derived his knowledge from personal inquiry, "is infested with autumnal fever, beyond, perhaps, any other portion of the

great valley." The following is his description of the country:

"The general surface of this cretaceous formation, near its northern margin, is hilly; further south, and in the neighborhood of the rivers, it is somewhat rugged, with extensive intervening plains, or table lands, from which the water, in many places, runs off with difficulty; and hence sluggish and swampy streams are not uncommon. As the strata are soft, the rivers, both small and large. have formed wide valleys, most of which are subject to inundation. Although the rocks, geologically speaking, belong to the cretaceous formation, no chalk has yet been discovered. The chief deposits are calcareous, and called by the people 'rotten limestone.' They consist of an argillaceous, friable carbonate of lime; but there are, likewise, soft sandstone, and strata of clay, sand, and gravel, of which some details may be given hereafter. Where the last two are at the surface, the rains percolate into the earth, and form permanent springs of pure, soft water; but where the rotten limestone shows itself at the surface, as it is free from fissures, and impervious to water, the springs are few, superficial, and transient."

The water of this region is not only of a bad taste, but seems to be positively noxious. Dr. Strudwich, of Demopolis, mentioned to Dr. Drake, that on a plantation of his, the operatives, who had been sickly under the use of the water of a "sipe-well," became healthy as soon as he had made an Artesian fountain.

The tertiary formation appears to be more healthy than the cretaceous, for the reason, perhaps, that more of its surface consists of sand, and being dry and sterile is wanting in the elements of malarious disease. But where the country is fertile, fevers of the worst type prevail. Probably no river in the great valley is more sickly than the Alabama, which runs through the cretaceous and tertiary formations.

Pascagoula river runs in all its course through tertiary

formations. Its mouth is in latitude 30° 30′, and its extreme sources are in 32° 40′ north. Speaking of that part of the country on the Pascagoula which lies near the Gulf, Mr. Darby says: "its unfruitfulness is counterbalanced to the inhabitants by the health they enjoy." According to the testimony of all, the basin of this river is but little affected with the fevers of autumn, and in its salubrity, affords, as remarked by our author, "instructive evidence of the connection, in the manner of cause and effect, between the topographical condition of a southern region and its bilious fevers."

Dr. Drake concludes his account of this part of the valley with the following general statements:

"1. The whole region which has been surveyed, lies south of the thirty-seventh degree of north latitude, and its broadest part is in the thirty-second; it is, therefore, a southern region.

"2. Its elevation above the Gulf of Mexico is but small, not averaging more than four hundred feet.

"3. It has an inclination to the south; which is true even of those parts which discharge their waters westerly and south-westerly into the Mississippi.

"4. It is composed of the (geologically) recent cretaceous and tertiary formations, which are friable in texture and miscellaneous in composition, still containing remains of organic matter.

"5. As a consequence of this structure, its streams have wide alluvions, and sluggish currents, which lead to frequent valley-inundations.

"6. It is undeniable, that this great region is more generally and seriously infested with autumnal fever, than any other portion of the interior valley of North America.

"7. Most of it has been settled within the last thirty years, and new plantations are still forming.

"It is, therefore, what we, provincially, call a new country, and with the progress of cultivation, may become much healthier. Most of its streams, however, will long continue to overflow their low, broad, alluvial bottoms, and thus thread the higher lands with lines of pools and swamp, which, under the influence of a southern sun,

will of necessity send forth the efficient cause of autumnal fever. The question, moreover, may be raised, whether, in summer and autumn, there may not be telluric emanations, from the unconsolidated strata of the comparatively recent tertiary and cretaceous formations, which the older carboniferous, Devonian, and Silurian formations do not send forth. I would not venture to answer this question affirmatively; but if such be the case, the region we have surveyed is permanently exposed to an additional cause of insalubrity."

The next section reviewed is the Ohio basin, which he thus contrasts with the preceding:

"The Ohio basin differs, in many respects, from the more southern basins over which we have traveled. Its general elevation above the level of the sea, excluding its mountains, is more than twice as great as the regions we have just left—that is, from seven hundred to one thousand feet; while the mountain borders to the east and south-east, rise from two thousand five hundred to five thousand feet. The north-west portions, in Illinois, Indiana, and Ohio, embrace tracts of level land, not unlike the plains of Alabama; but south of the Ohio river, the surface is everywhere ridgy, rising eastwardly into the mountainous; a character which belongs equally to the eastern portions of the region north of the Ohio.

"Geologically, the difference is equally great. A very small part, near the mouths of the Ohio and Tennessee rivers, presents the cretaceous formation, on which we have dwelt so long; all the rest offers at the surface older geological formations. The western, southern, and eastern parts embrace extensive coal deposits, with their accompanying sandstones, shales, and limestone; the central portions show at the surface Devonian sandstones and shales, of an older geological date; and large tracts of Silurian limestone, still older in the geological series, are found at the surface; the carboniferous and Devonian formations seeming to have been washed away. The oldest of these Silurian or transition rocks bulge up in, and a little south of, what has been designated as the geographical center of the basin. The rocks of these various formations are firmly indurated, compared with the cretaceous and tertiary deposits, and have, therefore,

been acted upon much less in a lateral direction. Hence most of the valleys are mere ravines, compared with those which have been excavated through loose and friable strata. This is especially true of that part of the basin which lies south of the Ohio river. To its north, however, in Illinois, Indiana, and Ohio, there are deep and extensive deposits of drift or diluvium, in which the streams have excavated wide valleys, and formed alluvial bottoms of corresponding breadth. All parts afford springs, though not of equal copiousness and permanence, and well-water can everywhere be obtained. The creeks and rivers rise rapidly; and the range between low and high water is great. In the northern margin of the basin, there are many small lakes, or, more properly speaking, large ponds, and numerous swamps of still greater area; but in the basin generally they are not found."

But we must quit topography, and proceed to notice the climate of the valley.

Climate, in physical geography, expresses a zone of the earth "parallel to the equator, and of such a breadth that the longest day in the parallel nearest the pole is half an hour longer than that nearest the equator;" but in etiology the term simply expresses states of the atmosphere. These, as enumerated by Dr. Drake, "consist in varying quantities or qualities of certain elements of the air itself—its caloric, light, and electricity; its aqueous vapor, fogs, mists, and clouds; its dews, rain, hail, frost, and snow; its weight and density; its movements or winds; its factitious gases, and mechanical impurities; all of which," as he correctly remarks, "may be very different in different times or places of the same geographical climate, and nearly the same in different zones."

We have alluded to the length of that part of this volume which treats of the climate of the great interior valley. We have no doubt that the subject is discussed at greater length than many readers would have chosen; but there is this to be said of it, that the laws of climate over

a field so vast as this valley could not be developed in a few pages. It has been the aim of the author to ascertain and unfold these laws throughout the whole extent of this wide region, and the interesting result is brought out, that almost every physician, knowing his latitude, longitude, and elevation, with this volume in his hand may estimate, without himself making observations, the character of the climate in which he lives. It will be admitted that such a result was worth a good deal of toil, and that a book which contains such a formula merits the favor of every scientific man.

The temperature is shown by meteorological tables kept at various posts, from north latitude 10° to 74°. According to these tables, the ratio of decrease of mean temperature is not uniform as we go north from the equator. Assuming with Humboldt, that 82° is the mean heat of equatorial regions up to 10° north, the decrease from this latitude to Melville Island, the mean annual heat of which is more than one degree below zero, ought to be 1° .28 for each degree of latitude; but there are many exceptions to the law. In some places, the decrement is only thirty-three hundredths of a degree of temperature for every degree of latitude; while in others it is 2° .04, 2° .10, 2° .17, 2° .28, and 2° .67. But taking all the data, Dr. Drake has been able to construct a theoretical table of mean temperatures which corresponds very closely with the tables made out from observation. In making the calculation, he employs three decrements of temperature; the first, for the equatorial belt of thirty-three hundredths of a degree for every degree of latitude; the second of 1° .16 (one degree sixteen hundredths) for the region extending from the equatorial belt to New Orleans; and the third of 1° .655 (one degree six hundred and fifty-five thousandths) for the region lying between 30° and 48° north.

In reference to the value of his calculations Dr. Drake remarks:

"Although these tables present only approximations to the truth, they will not be found useless to those who live in places where no thermometrical observations have been made. Should the reader wish by their aid to determine (approximately) the mean temperature of any particular spot, he must know its latitude, and, if it be beyond the forty-eighth parallel, its longitude, and in all cases its probable elevation above the level of the sea; when, by a resort to the ratios of the table, he can readily make the calculation. In the first section (within the tropics) one minute of latitude diminishes the temperature 0° .0055 (fifty-five ten thousandths of a degree,) which being multiplied by the number of minutes which his station lies above any given latitude of the table, will show him what deduction to make from the temperature of that latitude. For example, the temperature of the twentieth parallel, is 78° .82; if it be required to know the temperature of N. Lat. 20° 45′, the ratio for one minute, .0055, must be multiplied by 45′, which gives .25, which being substracted from 78° .82, leaves 78° .57 as the amount sought.

"In the second section of the table, the ratio of 1° .16 for a degree of latitude, gives 0° .193 (one hundred and ninety-three ten thousandths of a degree) for every minute; which must be, as in the other case, multiplied by the number of minutes of any particular station above a given parallel, and the product subtracted from the temperature of that parallel.

"In the third section of the table a degree of latitude reduces the mean heat 1° .655, which is, for each minute, 0° .0276 (two hundred and seventy-six ten thousandths,) to be multiplied, as in the other cases, by the number of minutes from the nearest parallel below, and the amount subtracted from the heat of that parallel.

"But in all cases allowance must be made for the difference of altitude. In the first two sections of the table, the ratios refer to the level of the sea; in the third, to an average elevation of six hundred feet. Let us take an example. Pompey, in N. Lat. 42° 56′, has, from observation, a mean temperature of 42° .84, which is 6° .03 less than that from calculation; but the altitude of Pompey, one thousand three hundred feet, or seven hundred feet above the average, reduces its temperature 3° .50, and this being added to the temperature from observation, raises it 46° .34." *(To be concluded.)*

THE WESTERN JOURNAL.

THIRD SERIES.—VOL. VI.—No. IV.

LOUISVILLE, OCTOBER 1, 1850.

UNIVERSITY OF LOUISVILLE.

It affords us sincere pleasure to state, that Dr. Drake returns to the chair in the University of Louisville of which he was so long the incumbent. Our readers, we are sure, will be gratified by this announcement. The institution is fortunate in having secured the services of a teacher so widely and favorably known. Dr. Drake's work, which has been noticed in our Journal, will strengthen the desire of students to hear his lectures on the Theory and Practice of Medicine. It is a compliment due to the author of this great work, that he should be recalled to the school in which he has spent some of the best years of his life. He will be cordially welcomed back by his old colleagues, and by his former pupils, many of whom have returned, and will be in the College hall to receive him on the first day of November.

We are also gratified in being able to announce, that Dr. Paul F. Eve takes the chair of Surgery in the above institution. Dr. Eve, too, has a wide reputation. He has been for a number of years connected with the Medical College of Georgia, and is distinguished as a teacher and a surgeon. We have had more than one occasion to notice the

operations of Dr. Eve, and have repeatedly alluded to the *Southern Medical and Surgical Journal*, which he edited for several years with marked ability.

The friends of the University cannot but be gratified in seeing the places of Bartlett and Gross filled by successors so worthy as Drake and Eve. Y.

PROLONGED LECTURE TERMS.

We have repeatedly expressed our views in opposition to the lengthening of the lecture terms in medical schools, beyond four months, and the opposition is based upon the belief that there may be such a thing as too much lecturing. That more can be said in six months than in four is undoubtedly true; but that more can be said profitably to students in the long term, than in that which custom and experience have prescribed we are not prepared to admit. The principle upon which this opinion is based is a very obvious one. Sir Walter Scott bears testimony to a kindred one, when he says that one reason why boys of limited means of acquiring knowledge make themselves the greatest masters of it, is that they are restricted to a few books, upon which they concentrate their powers, instead of dividing their energies over a multiplicity of means as boys with better resources do. And thus it is with medical teachers and pupils, as a general rule—the four months' term makes consciseness on the part of the teachers, and concentration on the part of the pupils, essential elements of a course, and much would be lost on both sides by the six mouths term. A recent lecture by Mr. Guthrie, at the Westminster Hospital clearly sustains this view. That gentleman has had ample experience in the subject of which he speaks. He is a veteran in the profession; from an early period of his life he has been engaged in arduous duties, and enjoyed, under Wellington, an ample field for observation. He became a member of the Council of the Royal College of Surgeons in 1824, of which body he has twice been President, and, during five years, filled the office of Professor of Anatomy and Surgery in that College. He has lectured on surgery, in London, nearly forty years. This venerable man, ripe with the richest experience, and full of the amplest knowledge, in a recent lecture at the Westminster Hospital, "on some of the more important points in surgery," says: "I think, gentlemen, you have enough for one day,

and more particularly if you will permit me to say, without intending offence to any one, whether teacher or student, that few, very few, of the candidates for the diploma of the College of Surgeons, who appear before me, as one of their examiners, know scarcely anything of what I have been explaining to you. Indeed, if you will further permit me, I will hint to you, with the greatest possible delicacy, that very few gentlemen of the present day appear to pay that attention to their studies, they did formerly, and the consequence is, that there are many not so well informed as students were twenty years ago, although three years of hospital and anatomical study are now required, instead of two."

When students concentrated their energies upon the course of two years, they were economic of time, but when they had three years' probation to undergo, they were prodigal of time, and economic of labor, and the result is as Mr. Guthrie details it. We are tempted to believe that a similar result would follow the lengthening of the lecture terms to six months. B.

The New York Register of Medicine and Pharmacy. Edited by C. D. Griswold, M. D.

We have received the first number of a new Medical Journal, of the above title. It is published in the city of New York, in semi-monthly numbers, of sixteen pages, at one dollar per annum. As a laborer in the advancement of medical science, and sound practical knowledge, we greet this new candidate cordially, and wish it an abundant success. B.

The Western Medico-Chirurgical Journal. Edited by J. F. Sanford, M. D., and Samuel. G. Armor, M. D., Professors in the Iowa State University.

The Far West is rapidly growing in population, and the young State of Iowa, already boasts a medical school, and a medical journal devoted to the interests of science.

The two numbers of this new journal, the first and second, which

we have received, speak favorably of the abilities of the editors, and we hope they will succeed well in the responsible duties they have undertaken. B.

MEDICAL TEACHING IN NEW YORK.

The New York Register of Medicine and Pharmacy indulges the belief, that recent movements in the city of New York, "will do much towards making that city, what it should be: the metropolis of medicine, as it is of other interests." We are inclined to think that the belief is more a matter of hope, than faith, and we are satisfied that it is a hope that will not soon grow into fruition. In the nature of things it is impossible that anything like centralization can flourish under the political institutions of this country, and the diversified climates, habits of the people, and peculiarities of sections, in this country, form an insurmountable barrier to centralization in medical teaching. In the contest for supremacy, the West is abundantly able to maintain that proud position in medical science, she has held for thirty years. From a long and intimate acquaintance with the subject, we unhesitatingly affirm, that in no part of the Union has the science of medicine been better taught, than in the West, and so far from having lost any of her former elements of capacity for medical teaching, we feel well assured that her teachers have kept pace, with those of the East. Her pupils are scattered over the West and South, and will compare favorably with practitioners of any school that may be named. Those who live among the people of the West, who draw their observations from actual life, and who know the modifications from an Eastern standard, necessary to be made in the treatment of the diseases of the West and South, are better prepared to teach, than those, who aspiring to teach, have first to be taught. Without wishing in the slightest degree to disparage the schools of the city of New York, we feel that we do nothing more than justice to truth in saying, that Louisville is better qualified for making successful practitioners in the South and West, than New York can possibly be. The students see in the Louisville Hospital, the modifications of disease, that are the natural product of peculiar climate and habits of living, and they are taught the means of treating these modifications by those who have experience. Hospital facilities, extensive library privileges, and the instruction of native teachers whose qualifications are ample, should

equip students fully for the successful practice of their profession, and in the discharge of the duties of practice they should have as little to unlearn as possible.

But it may be urged that one of the New York schools has selected two Western professors as teachers. This, however, does not militate against the doctrine we are endeavoring to establish. The hospitals in New York are Eastern institutions; the diseases found in them vary widely, in many of their forms, from the maladies that are indigenous to the West, and the teaching must be done with classes, a large majority of whose numbers are Eastern men. The West has long ceased being much of a tributary to the medical schools of the East, and the practice of sending young men from the West, to be educated in the East, as physicians, is rapidly diminishing. It is considered almost essential that a Western lawyer shall be prepared for the practice of his profession, at home, and the reasons that require the same for the physician are quite as imperative. The forms of practice in both professions, being practiced at home in a peculiar way, are best taught at home. B.

PRACTICAL MEDICINE.

We are preparing a series of papers on some of the principal articles used in the practice of medicine, and shall commence their publication in the November number of this *Journal*. B.

ANATOMICAL DRAWINGS.

We regard good anatomical drawings as the means nearest the excellence of actual dissections, for imparting sound and valuable knowledge to students of medicine, and in the term student we do not mean merely the neophyte in the approaches to the temple, but all those who, after attaining high distinctions in all the departments of medicine, continue to press onward and upward in the pursuit of knowledge. We have been reminded of the value of anatomical drawings, by a recent examination of several very superior specimens, drawn and colored by Mr. Charles Wolford, a very tasteful and skillful artist of this city. These specimens were prepared for Dr. Gross, and Professor Miller has, in process of execution, some beautiful drawings, for his department.

They are executed in handsome style, and cannot fail to give accurate and impressive anatomical information to the Professor's classes. The artist deserves much praise for the admirable manner in which he has accomplished the orders of his medical employers, and we hope his useful art will not be suffered to languish for the want of patronage. A series of enlarged drawings of Maclise's anatomical plates of the anatomy of hernia, would be invaluable in a course of surgical teaching, for it would give students a much more accurate idea of the anatomy of hernia, than is imparted by any process of didactic teaching we are acquainted with. B.

THE WESTERN LANCET, AND THE MEMPHIS SCHOOL ADVERTISEMENT.

Our friends of the Western Lancet express some concern respecting the advertisement of the Memphis School, on the cover of this Journal. There should be some discrimination in these censures, and a little examination would have taught the Lancet, that the editors of the Western Journal are not the owners of the work. The advertising is the property of the proprietors of the Journal, and is a matter upon which the editors are not consulted. It will be time enough to censure the editors, when the Memphis School figures in the contents of the Journal. B.

THE MEMPHIS SCHOOL.

The Western Lancet denounces Dr. James C. Cross for his connection with the Memphis School; we cannot join in this denunciation, because we think Dr. Cross may be safely left to the enjoyment of his position. B.

CHLOROFORM.

In the Dublin Quarterly Journal, August, 1850, Dr. Beatty publishes an excellent article on the use of Chloroform, which we intended to quote among our extracts in the present number of the *Journal*. But, as we had no space to spare for selections, we must be content with the following remarks of Dr. Beatty, on a paper, read before the Royal Society of Edinburgh, by Dr. Gregory, in March, 1850. Dr. Beatty says:

"The paper is of great value, and should be carefully studied by all who are interested in the success of anæsthetic treatment. He ascribes the injurious effects of the chloroform in ordinary use to the presence of certain volatile oily impurities, which must be removed before it can be safely employed. These oils contain chlorine, have a disagreeable smell, and when inspired or smelt cause distressing headache and sickness. It is, therefore, highly probable that when these symptoms occur, as they do with some individuals, from the use of chloroform of more than average goodness of quality, they depend on the presence of a trace of these poisonous oils. The test which Dr. Gregory recommends for these impurities, is agitation of the chloroform with sulphuric acid, which should be quite colorless, pure, and of the full density of 1.840 at least. This, when agitated with the impure chloroform, becomes yellow or brown, from its action on the oils, which it chars and destroys. Any change of color is easily seen by the contact with the colorless chloroform that floats above. Pure chloroform gives no color to the acid. As this is a subject upon which too much stress cannot be laid, I will beg leave to transcribe Dr. Gregory's instructions for the purification of the adulterated drug:

"The chloroform having been tested as above, and found more or less impure, it is to be agitated with the oil of vitriol (half its volume will be sufficient) and allowed to remain in contact with the acid, of course in a clean, dry, stoppered bottle, and with occasional agitation, till the acid no longer becomes darker in color. As long as the action is incomplete there will be seen after rest at the line of contact a darker ring. When this no longer appears, the chloroform may be drawn off, and for greater security may once more be acted on by a quarter of its volume of the acid, which should now remain colorless. It is now once more to be drawn off, and in a dry, stoppered bottle mixed with a little powdered peroxide of manganese, with which it is gently agitated, and left in contact until the odor of sulphurous acid is entirely destroyed, and the chloroform has acquired a mild, agreeable, fruity smell. It has then only to be drawn off into a proper phial. It will now leave no disagreeable smell when evaporated on the hand.

"Mr. Kemp has observed, in repeating this process for me, the very curious fact, that as soon as the action is complete, but no sooner, the chloroform, tested with the acid in a tube, exhibits a strongly convex surface downwards, where it rests on the pure acid, or what is the same thing, the acid becomes concave at its upper surface. The smallest trace of impurity, not sufficient to effect the density of the chloroform, we have found to render the line of junction horizontal."

THE LARGEST LIBERTY.

The radical politicians of the day have in much vogue among them the elastic phrase we have adopted as the caption of these remarks, and we cannot say that we feel much surprise in finding that the spirit, at

least, of the phrase has crept into the medical politics of an establishment called the Memphis Medical Institute. The Trustees of that institution, with an admirable conception of the responsibilities of the trust bestowed upon them, have recently passed the following catholic views of medical teaching:

"Resolved, That this school of medicine is not to go under any sectarian or peculiar denomination; is not to teach any peculiar system of medicine; but all that is believed will throw light on the nature of disease, or contribute to its alleviation or cure,—and that it professes to be orthodox.

"Resolved, That each Chair will be expected to inculcate the doctrines which its Professor holds to be scientific truth; and that Professor Cross will teach the Institutes of Medicines as his convictions present, past, and future shall deem sanctioned by the light of science, and his own high reputation and exalted rank in the profession, and as most conducive to the well being and character of the Memphis Institute. To each Professor should belong the right to teach his own views and opinions connected with the branch of Medical Science committed to his Chair.

"Resolved, That in creating the new chair, over which Profestor W. Byrd Powell is to preside, it shall be designated as that of Cerebral Physiology, Mineralogy, and Geology, and in which Professor Powell shall have the privilege of treating of the external senses, and of so much of the nervous system in general, as he may deem requisite to a proper understanding of the functions and pathology of the brain."

If the Memphis Institute does not flourish like a green bay tree, under the auspices of such liberal concessions on the part of the Trustees to the Faculty, the Governors may feel assured that the fault does not lie at their door. The liberality extended to Professor Cross would seem to be the *ultima thule* in that line, until the reader finds himself involved in the labyrinths of the Professor Byrd Powell resolution. The beautiful harmonies, the Mosaic work of that gentleman's chair, that etherial body of light, known as Cerebral Physiology, surrounded with the tesselated border of Mineralogy and Geology, needed the expounding abilities of the trustees, and we are free to confess they have reached the usual result of commentaries,—by making confusion worse confounded. We had a dim, shadowy idea of Professor Byrd Powell's chair until we read the exposition by the trustees—from that moment, we realised the sensation of darkuess that may be felt. The trustees owe it to themselves, and to the institution over which they preside, to swing out, in front of their Medical edifice, a sign announcing "entertainment for all kinds of travellers." B.